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PSYCHOLOGY AND PHILOSOPHY

I.—THE CONTRARY-TO-FACT CONDITIONAL.[1]

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I.

A significant part of our knowledge is usually expressed in subjunctive and "contrary-to-fact" conditional statements. We seem to have knowledge of what *might* have happened, of what *would* happen if certain conditions were realized, of what tendencies, faculties, or potentialities an object *could* manifest in suitable environments. And this, most of us would be inclined to say, is valid and significant, even though the possible events to which it seems to pertain may never become actual. The type of statement in which this knowledge is usually formulated, however, appears to have been by-passed by contemporary logic; for the theories of generality, implication, and "statement composition", as they have been developed in recent years, seem to concern only indicative statements and to make no adequate provision for what we usually express in the subjunctive. Our problem here is to determine whether there is any other means of expressing this important counter-factual information. As we shall see, the philosophical problems which this question involves are fundamental to metaphysics, epistemology, and the general philosophy of science.

Many contrary-to-fact conditionals are not expressed in the subjunctive mood and many conditionals which are expressed in this mood are not actually contrary-to-fact, but in the present

[1] I am much indebted to W. V. Quine, with whom I have discussed this question at length. He should not be held responsible, however, for any of my remarks.

discussion we may use the labels "subjunctive conditional" and "contrary-to-fact conditional" interchangeably. Neither term is adequate, but each has been used in recent literature. The essential characteristics of this important type of statement will be more clearly delineated as we proceed.

There is a variety of types of situation where the use of the contrary-to-fact conditional appears to be the most natural means of expressing what we claim to know. First of all, of course, there are those occasions where we assert a conditional statement, knowing or believing its antecedent to be false. I may contend, for example, that had we followed a different policy toward Germany in the 1920's, the second World War would not have occurred. If this contention is correct, it must be considered, along with all other true and relevant opinions, in any reasonable discussion of contemporary policy. In general, it may be said that adequate understanding of science and history requires the ability to consider the consequences of hypotheses known to be contrary-to-fact. In the study of anatomy, for instance, it would be difficult to assess the importance of an organ or function unless we were able to conceive what would happen if that organ or function did not exist. In physics it is necessary to be able to conceive of states of affairs which, in all likelihood, will never become actual. Thus Galileo, as is well known, founded his dynamics upon the conception of a body moving without the influence of any external force. Examples of this sort may be readily multiplied.[1]

Equally important, from the point of view of knowledge, are those subjunctive conditionals which we assert, not knowing whether the antecedents are true or false. C. I. Lewis has emphasized that it is only by means of such conditionals that we can adequately express the reasons behind our precautionary activity; it is essential for a being who is active that "there should be 'If—then—' propositions whose truth or falsity is independent of the truth or falsity of the condition stated in their antecedent clauses".[2] I try to avoid falling through the ice because I believe that if I were to fall I should get wet. Since I believe the conditional to be true, I endeavour to prevent the realization of the conditions mentioned in the antecedent.

[1] *Cf.* Ch. V, "Facts and Ideals", *An Essay on Man*, by Ernst Cassirer; M. R. Cohen, *Reason and Nature*, p. 69; C. G. Hempel, "Studies in the Logic of Confirmation", MIND, Vol. LIV, No.'s 213 and 214 (1945), esp. p. 16.

[2] C. I. Lewis, *Mind and the World Order*, p. 142; *cf.* pp. 140-142, 182-183. *Cf.* the same author's "Meaning and Action", *Journal of Philosophy*, Vol. XXXVI, No. 21. *Cf.* F. H. Bradley, *Logic*, Part I, Ch. II.

Bradley's "All trespassers will be prosecuted" is a further example: what this intends to convey is that if anyone were to trespass he would be prosecuted—and the message is usually posted conspicuously in order to insure that it remain contrary-to-fact.

Still another use of this type of conditional is what has been called its "deliberative use".[1] When we prepare for a crucial experiment, we review the situation and consider what would happen if our hypothesis were true and what would happen if it were false. The subjunctive conditional is essential to the expression of these deliberations. In defending a hypothesis, I may employ a subjunctive conditional even though I believe the antecedent to be true; I may say, "If this were so, that would be so; but, as you see, this *is* so. . . ." It is said that detectives talk in this manner. Whenever we modify our conditional assertions "for the sake of argument", withholding commitment concerning the truth or falsity of their antecedents, we find ourselves falling into the subjunctive.[2] Similarly, in order to falsify a theory or reduce it to absurdity, we must be able to consider its consequences in a conditional, the component truth-values of which we deliberately ignore and which, therefore, we should normally express in the subjunctive.[3]

This type of conditional is implicit in the use of what Broad and Carnap have called "dispositional adjectives" or "disposition terms"—terms such as "malleable", "fragile", "soluble" and so on—which are used when we want to refer to the dispositions or potentialities of a thing.[4] Broad has pointed out that "whenever we conjoin a dispositional adjective to a substantive we are expressing in a categorical form a hypothetical proposition of the following kind. 'If this *were* in a certain state, and *were* in certain relations to certain other things of certain specified kinds, then certain events of a specific kind *would* happen either in it or in one of these other things'".[5] To say that a thing is fragile, for instance, is to say that if certain conditions were realized it would break. To say that an acorn is potentially an oak is to say at least that, under certain conditions which may or may not become actual, it would grow into an oak. This potentiality increases with the likelihood of the conditions being realized.

[1] Roderick Firth, *Sense-Data and the Principle of Reduction*, Ph.D. Thesis, Harvard University Library, 1943, ch. VII. [2] *Ibid.*

[3] *Cf.* Bertrand Russell, *Introduction to Mathematical Philosophy*, p. 161.

[4] C. D. Broad, *Examination of McTaggart's Philosophy*, Vol. I, pp. 148 ff.; R. Carnap, "Testability and Meaning", *Philosophy of Science*, Vol. 3, No. 4 (1936), pp. 419-471, and Vol. 4, No. 1 (1937), pp. 1-40.

[5] *Op. cit.* p. 149. Italics mine.

To say that an individual is predisposed toward psychoneurosis is to say that under certain conditions he would become neurotic and possibly also that under those conditions a normal individual would not become neurotic. This notion of "disposition" is central to the ancient philosophical questions concerning possibility and potentiality.

There are many other important philosophical theories whose central tenets depend upon the admissibility of what is formulated in the contrary-to-fact conditional. Four instances may be cited from contemporary philosophy. (1) The phenomenalist maintains that, apart from those sense-data which are objects of actual experience, the ultimate constituents of the universe are what H. H. Price has called "hypothetical sense-impressions", sense-data which *would* become actual if certain other sense-data *were* to become actual. Indeed, Price notes that "the phrase 'hypothetical sense-impression', in fact, is just an abbreviation for a hypothetical *statement* of the form: if so and so were the case, such and such a sort of sense-impression would exist".[1] (2) The essence of pragmatism, as well as of "realism", according to C. S. Peirce, is to make "the ultimate import of what you please to consist in conceived conditional resolutions, or their substance".[2] These conditional resolutions are formulated in subjunctive conditionals which state what the "real generals" of the universe "*would* or might (not actually *will*) come to in the concrete".[3] (3) Much of contemporary analytic philosophy seems also to involve these conditionals, although somewhat less explicitly. The subject-matter of this philosophy comprises statements or sentences, so that a philosophical assertion may be a statement such as "The scientist compares his hypothesis with the protocol statements". Inasmuch as many, if not most, of the statements thus mentioned, are never actually uttered, discussion of them seems to presuppose an implicit use of the subjunctive (concerning what the scientist *would* state if he *were* to formulate his observations, etc.). Thus Carnap tells us that, implied in his notion of "protocol statement", is "a simplification of actual scientific procedure *as if* all experiences, perceptions, . . . etc., . . . *were* first recorded in writing as 'protocol' to provide the raw material for a subsequent organization".[4] (4) Finally it is significant to note the extent to which those logicians who have not explicitly sanctioned the use of such

[1] H. H. Price, *Hume's Theory of the External World*, p. 179. *Cf.* A. J. Ayer, *Language, Truth and Logic*, pp. 75 ff., esp. p. 78; *The Foundations of Empirical Knowledge*, Ch. V. [2] C. S. Peirce, *Collected Papers*, 5. 453.
[3] *Ibid.* 6. 485. Peirce's italics. *Cf.* 5. 526, 5. 517, 3. 526 ff.
[4] R. Carnap, *The Unity of Science*, p. 43. My italics.

conditionals are apparently unable to avoid falling into that mode of speech in the formulation of crucial points in the logic of science.[1]

It is clear, then, that the subjunctive or contrary-to-fact conditional seems to be required for the formulation of important assertions which are constantly made in philosophy, science, and ordinary discourse. Although there is extreme difficulty involved in analysing the meaning of these assertions, we are not thereby justified in dismissing counter-factual questions as "pseudo-problems" or in concluding that the contrary-to-fact conditional does not say anything.[2] We may agree with Broad that the distinction between what will be and what would be must in some sense "correspond to something real" and that philosophy cannot afford to ignore it.[3] In the present paper, I shall try to make some progress toward clarifying and solving this problem.

II.

Our problem is to render a subjunctive conditional of the form, "$(x)(y)$ if x were ϕ and y were ψ, y would be χ", into an indicative

[1] Instances may be drawn from the writings of the logicians referred to thus far who fit in this category. Carnap, "Testability and Meaning": "If we knew what it *would* be for a given sentence to be found true then we *would* know what its meaning is. . . . We call it [a sentence] *confirmable* if we know under what conditions the sentence *would* be confirmed. . . . A sentence may be confirmable without being testable; *e.g.* if we know that our observation of such and such a course of events *would* confirm the sentence, and such and such a different course *would* confirm its negation without knowing how to set up either this or that observation". (pp. 420-421). Carnap takes "observable" and "realizable" as basic descriptive terms in his theory of empiricism (p. 454). Like "confirmable" and "testable" these are disposition terms and thus may be said to furnish abbreviations for subjunctive conditionals.

Hempel, *op. cit.* p. 109; "The concept of the development of a hypothesis H, for a finite class of individuals, C, can be defined [as] what H *would* assert if there existed exclusively those objects which are elements of C". *Cf.* also pp. 2, 25.

Russell, *Inquiry into Meaning and Truth*, pp. 278-279. An "unexperienced percept" is what "*would* verify 'ϕa' if we could assert 'ϕa'. But we cannot assert it. . . ." *Cf.* also pp. 250, 281, 320, 350.

The italics in the above quotations are mine.

[2] *Cf.* E. Mach, *Die Mechanik in ihrer Entwickelung*, 1st ed., p. 216. Mach held that it is always invalid to argue on the basis of an assertion about what-would-have-happened-if. It is of historical interest to note Russell's early repudiation of Mach's view in *The Principles of Mathematics*, pp. 492-493. A recent dismissal of counter-factual questions as "pseudo-problems" occurs in a review by Robert Eisler, of *The Philosophy of Bertrand Russell, Hibbert Journal*, Vol. XLIII, No. 3, p. 283.

[3] *Op. cit.* Vol. I, p. 264.

statement which will say the same thing. Some subjunctive conditionals are simpler, *e.g.* they may be of the form, "(x) if x were ϕ, x would be ψ", or "if a were ϕ, a would be ψ" (where "a" represents a proper name); and some are more complex. But we shall find that the problem is the same in principle, whatever the complexity of the conditional. Like Russell in his theory of descriptions, we want to find a new way of saying something—in this case, in order to assure ourselves that we *can* restate what we ordinarily express in subjunctive conditionals. The problem is epistemological and metaphysical, as well as logical and linguistic; we want to know what it is, if anything, that we have to assume about the universe if we are to claim validity for our counter-factual knowledge.

There appears to be no problem connected with those subjunctive conditionals which are logically true (*e.g.* "If wishes were horses, wishes would be horses") or those which are analytic (*e.g.*, "If that animal were a quadruped, it would have four legs"). Hence, in what follows, all reference to subjunctive or contrary-to-fact conditionals should be understood to intend only those which are not analytic or logically true. Similarly, any reference to statements or to specific types of statement should be understood to intend only *indicative* non-counter-factual statements, unless qualification is made. At the present stage of the discussion we shall leave undecided the question whether statements name (or, in any sense, refer to) propositions.

The simplest methods of translating these conditionals are clearly inadequate. Consider this example: "If the vase were dropped to the floor, it would break". Is an adequate translation yielded by replacing the 'were' and 'would' by 'is' and 'will' and interpreting the statement as a truth-functional material conditional? If it is a material conditional, there can be no doubt of its truth, for (let us assume) the vase will never have been dropped to the floor. Such conditionals are true when their antecedents are false. This becomes more evident when we transform the conditional into an alternation, which is another means of expressing the same thing: "Either the vase will not be dropped on the floor or it will break". On similar grounds, this material conditional is also true: "If the vase is dropped on the floor, it will grow into an oak". But this conditional, we may agree, is not relevant in any discussion concerning the care of the vase. A material conditional seldom affords a ground for action unless one can assert the corresponding subjunctive. In the present instance, the corresponding subjunctive, "If the vase were dropped on the floor, it would grow into an

oak", is (according to all evidence) false. A subjunctive conditional cannot be transformed into a simple alternation and it may be false when its antecedent is false and may be false when its consequent is true. Therefore, since the subjunctive conditional may be true when the corresponding material conditional is not, and *vice versa*, we may conclude that the subjunctive cannot be thus simply rendered. As Lewis has put it, we want to be able to infer the consequent *hypothetically* from the antecedent; but, knowing merely that the antecedent of a material conditional is false (or that its consequent is true) and hence that the conditional is true, we cannot say that the consequent *would* be true if the antecedent *were* true.[1] A subjunctive conditional is one such that we can know that the antecedent in some sense implies the consequent without knowing the truth-values of either.

A similar objection may be made to the simple translation of a universal subjunctive statement. Consider: "(x) if x were a vase and were dropped to the floor, x would break". Interpreted as an indicative universal conditional (or "formal implication"), it would be true merely if no vases were ever dropped to the floor, for what such a statement really says is: "(x) either x is not a vase which is dropped to the floor or x breaks". On similar grounds we may assert, "(x) if x is a vase and is dropped to the floor, x bounces to the ceiling". In cases such as these, the universal statements may be said to be only trivially or vacuously true and, although admissible in logic, of little interest either in science or in ordinary discourse. The inadequacy of these types of statement is most apparent in science when we wish to make a universal statement which we believe to be without existential import—for instance, a statement about the behaviour of bodies which are freely falling, or are at absolute zero, or in a perfect vacuum.

Carnap has proposed a rather involved method of dealing with "disposition predicates" which might appear to be relevant to our problem.[2] He does not note that they involve an implicit use of the subjunctive (*i.e.* that they may be regarded as abbreviations of subjunctive conditionals), but he admits that apparently they cannot be defined by the usual techniques. Despairing of defining them, he offers another method of "introducing" them, *viz.* the use of "reduction sentences", which, he

[1] C. I. Lewis and C. H. Langford, *Symbolic Logic*, p. 261.

[2] "Testability and Meaning", pp. 440 ff. *Cf.* "Logical Foundations of the Unity of Science", *Encyclopedia of Unified Science*, Vol. I, No. 1, pp. 50 ff.

admits, can at best yield "a partial determination only".[1] Whatever its merits, however, this method is of little aid to us in our present problem.

A reduction sentence for the property Q_3 (*e.g.* soluble in water) is a statement that the conjunction in any object of two other properties—the "experimental situation" Q_1 (being placed in water at time *t*) and the "experimental result" Q_2 (dissolving in water at time *t*)—is a sufficient condition for the predication of the disposition term "Q_3", provided that the conjunction of Q_1 and Q_2 occurs at least once. In the simplest cases, the situation is such that the non-occurrence of Q_2 indicates that the thing in question does not have the property Q_3. A reduction sentence for any disposition term, then, is a sentence stating a *sufficient* condition for the application of that term, but it gives us a rule for applying the term only in those cases in which the sufficient condition (Q_1 and Q_2 in our illustration) is realized. We may state more and more sufficient conditions, but "a region of indeterminateness" will always remain—*i.e.* those cases where none of the sufficient conditions ever obtain. Thus Carnap admits that "if a body *b* consists of such a substance that for no body of this substance has the test condition—in the above example: 'being placed in water'—ever been fulfilled, then neither the predicate nor its negation can be attributed to *b*".[2] We are compelled to say that, in this "region of indeterminateness" where neither the predicate nor its negation may be applied, the disposition term has "no meaning".[3] In other words, instead of saying that our rare body *b* either is or is not soluble, we must say that it is *meaningless* to call it soluble (or insoluble). Even if this were consonant with actual practice, which seems at least doubtful, this conclusion would hardly be satisfactory. This is particularly evident in view of the fact that the statements "Body *b* is placed in water at time *t*" and "Body *b* dissolves at time *t*" (which would be the components of a reduction sentence pertaining to body *b*) are themselves perfectly meaningful.[4] Carnap's method, therefore, does not solve our problem, nor does it seem to be a completely satisfactory means of dealing with disposition terms.[5]

[1] "Testability and Meaning", p. 449. [2] *Ibid.* p. 445. [3] *Ibid.* p. 449.

[4] *Cf.* Firth, *op. cit.* Ch. VII. Firth's discussion, to which I am indebted, contains a penetrating analysis of Carnap's theory and its relation to the general problem of the contrary-to-fact conditional.

[5] Carnap does not discuss what would be the consequences for the philosophy of empiricism if this method were to be applied to the terms, "observable" and "realizable", which are the two basic terms of his

In *The Examination of McTaggart's Philosophy*, Broad proposed the view that a subjunctive conditional about what a particular entity might have been, or could be, should "be taken as an abbreviation" for a "statement about *any* thing of a given *kind*". A statement about the disposition of an individual then becomes a statement which says, among other things, "that, if at *any* time any substance of this kind were put into a situation which was any determinate form of S, its determinate behaviour would be . . ." etc., etc.[1] This view does not contribute toward the solution of our problem, however, since it merely reduces subjunctives about individuals to subjunctives about classes. Similarly, we may dismiss C. L. Stevenson's brief treatment of dispositions in *Ethics and Language* (pp. 46 ff.), since his account admittedly presupposes the notions of "cause" and "law". At the present stage of our discussion, these notions would, of course, be question-begging.

The most fruitful means of handling disposition terms, apparently, is to make explicit the subjunctive conditionals which they involve (*e.g.* "If body *b* were placed in water at time *t*, it would dissolve at time *t*") and then to consider these as instances of our more general problem. Given a method of treating the subjunctive, it may then be possible to throw light, not only upon disposition predicates, but also upon such notions as "law", "cause", "physical necessity", etc. These latter applications, however, will not be made in the present paper.

III

Let us now consider in detail the difficulties which are involved in the attempt to eliminate the contrary-to-fact conditional. We may proceed on the basis of certain suggestions made by F. P. Ramsey in his posthumous paper, "General Propositions and Causality".[2] Let us assume that my belief in the conditional, "If you were to see the play, you would not enjoy it", constitutes my principal reason for suggesting that you do not go. The situation may be described in this manner: I feel that you would be ill-advised in going, because I have (or believe I have) information which is such that from it and the hypothesis that you do see the play, I can derive the conclusion that you won't enjoy

"empirical methodology". ("Testability and Meaning", p. 454.) Applied to the former term, it might conceivably lend new support to the doctrine that to be is to be perceived.

[1] Vol. I, p. 276.

[2] Included in *Foundations of Mathematics*, pp. 237-257.

it. And if you should question my advice, our difference would most probably be with respect to this alleged information. Ramsey stated the essence of the matter: "In general we can say with Mill that 'If p then q' means that q is inferrible from p, that is, of course, from p together with certain facts and laws not stated but in some way indicated by the context. This means $p \supset q$ follows from these facts and laws. . . . If two people are arguing about 'If p will q?' and are both in doubt as to p, they are adding p hypothetically to their stock of knowledge and arguing on that basis about q".[1] What is the nature of the connection on the basis of which we derive q? We shall go astray if we confine ourselves to a search for the "connection" which must hold *between* p and q. This is confirmed by the fact that we affirm many subjunctive conditionals in order to show that there is *no* relevant connection between antecedent and consequent; *e.g.* "Even if you were to sleep all morning, you would still be tired". What then, is the nature of the "connection" which is involved and between what entities does it obtain?

W. V. Quine has suggested that possibly some "strong relation of statements" such as logical implication or entailment could be used when we want to formulate what is expressed in a subjunctive conditional.[2] An entailment, which may be interpreted as saying something *about* statements, does not involve the paradoxes of "vacuous truth" which we have considered in the cases of the material and universal conditionals and material and formal implication. Now it is obvious that the antecedents of most subjunctive conditionals do not logically entail the consequents, for in most cases (as in our example) there is no *contradiction* involved in denying one and affirming the other. We have, in fact, restricted ourselves in this discussion to a consideration of those subjunctive conditionals which are not analytic or logically true. But let us consider this along with the previous suggestion and look in another place for this "strong relation of statements". C. I. Lewis has pointed out that, when an inference is made in ordinary discourse, even though a material conditional may be involved, we are using an entailment of the form "p and $p \supset q$ logically imply q".[3] Let us consider, then, whether a subjunctive or contrary-to-fact conditional can be reformulated as an entailment stating that the consequent is

[1] *Ibid.* pp. 248, 247. *Cf.* A. Tarski, *Introduction to Logic*, p. 24; W. V. Quine, *Elementary Logic*, p. 24.

[2] *Mathematical Logic*, p. 29.

[3] *Cf.* C. I. Lewis and C. H. Langford, *Symbolic Logic*, pp. 242-246.

entailed by the antecedent taken in conjunction with a previous stock of knowledge.

Consider this conditional, *C*: "If Holbrook were elected, the price of wheat would rise." Is this another way of saying that the indicative statement "Holbrook is elected" (which we may call '*H*') in conjunction with certain previous information entails "The price of wheat will rise" (*W*)? First of all, it is necessary to revise the reference to "previous information", since the meaning of the conditional should not be confused with the particular grounds upon which it happens to be asserted. You and I may have quite different "stocks of knowledge" and affirm *C* on extremely divergent grounds, but when each of us does affirm *C*, we are, it must be assumed, saying exactly the same thing. The "something additional" which each of us adjoins to *H* in order to deduce *W* need not be a statement expressing any particular item in either of our stores of knowledge, nor indeed need it express any knowledge at all. When we assert a subjunctive conditional, we are saying something more general. In the present instances, we are saying that there is *some* true statement which, taken with *H*, entails *W*. If, knowing nothing about politics and economics, I none-the-less presume to conjecture that prices would rise if Holbrook were elected, I am conjecturing that there is some true statement, I know not what, which, in conjunction with *H*, entails *W*. If I knew *what* the true statement is, I could be said to have an *explanation* for the situation which *C* describes, but, obviously, I do not need to know such an explanation in order to know the *meaning* of *C*. It is quite possible that the statement may refer in part to some future events concerning which I shall never know anything.

May we conclude, then, that our conditional *C* is another way of saying: "There is a statement *p* such that *p* and *H* entail *W* and *p* is true"? This is a plausible suggestion, but a number of modifications must be made if it is to be satisfactory. It is necessary to place restrictions upon *p* so that there will be no possibility of finding a value for *p* which would trivialize the translation. Thus, if *p* included a statement which was vacuously true, the translation would not say enough. For instance, if we may suppose that Holbrook will never be elected to public office, then the universal conditional "(*x*) if *x* is a public office and Holbrook is elected to *x*, the price of wheat will rise" is vacuously true and its inclusion as a part of *p* would make our translation inadequate. That this is so becomes evident if we reflect that the universal conditional, "(*x*) if *x* is a public office and Holbrook is elected to *x*, this year's wheat will turn to

gold", is also vacuously true. Our formula, as it now stands, would require that, on the basis of this triviality, we assert the subjunctive conditional, "If Holbrook were elected, this year's wheat would turn to gold", which, we may assume, is absurd.

It is necessary to modify the formula in order to insure that it contain no "vacuous truths", *i.e.* in order to insure that it contain no universal conditional whose antecedent determines an empty class and no material conditional (or material implication) which is asserted merely on the ground that its antecedent is false (or its consequent true). Every universal conditional included in p must have "existential import", that is, every universal conditional must have conjoined with it a statement asserting that there are members of the class determined by the antecedent. Even this is not enough, however.

Suppose, for instance, we desired to translate our earlier example, "If you were to see the play you would not enjoy it", according to the formula thus restricted. Trivialization is still possible. Let p be "(x) [x = you : $\supset$: x saw the play . $\supset$. x did not enjoy the play], there exists an x such that x = you". This will be a true statement if the vacuous material conditional corresponding to the original subjunctive conditional is true (and, of course, whenever we can assert the subjunctive, we can also assert the corresponding indicative). The translated subjunctive will then become equivalent to the material conditional. To preclude this type of difficulty, we may add a further provision to our formula, without sacrificing anything essential. Let us say: p includes no universal conditional whose consequent includes any two functions which are logically equivalent to "x sees the play" and to "x does not enjoy the play"; *i.e.* any consequent must exclude either functions logically equivalent to "x sees the play" or functions logically equivalent to "x does not enjoy the play".[1] We may now state in more general terms the formula proposed:

A subjunctive or contary-to-fact conditional of the form, "$(x)(y)$ if x were ϕ and y were ψ, then y would be χ", which is not analytic or

[1] If we understand the term "entailment" in the very strict sense of logical implication, this provision will take care of our difficulty, but if, as is often the case, the term is construed in a wider sense, further modification is necessary. *I.e.* if we so construe it that "x sees the play" and "x witnesses the play" may be said to entail each other (on the ground that, although they are not logically equivalent in the strict sense, they are *synonymous*), the latter phrase may be substituted for the former and the translation trivialized as before. Hence, if we use "entailment" in the wider sense, we should substitute "synonymous" for "logically equivalent" in the provision. For a discussion of these terms, see W. V. Quine, *O Sentido da Nova Lógica*, pp. 148-152.

logically true, may be rendered as: "There is a a true statement p such that: p and 'x is ϕ and y is ψ' entail 'y is χ'; p includes no propositional function having free variables other than x and y which is not either a universal conditional or an existential statement; p includes no universal conditional which does not have existential import; and p includes no universal conditional whose consequent includes any two functions which are logically equivalent to functions having 'x is ϕ and y is ψ' and 'y is χ' as corresponding instances, or whose antecedent includes any function not containing the variable of quantification".[1]

There are more qualifications to be made. To preclude trivialisation in those cases where the consequent of the subjunctive conditional happens to be true, we should add that the indicative version of the consequent does not entail p. And some types of subjunctive conditional must be reformulated before the formula can be applied. *E.g.*, "Even if you were to sleep all morning you would still be tired". This type of statement is what one gets by negating the consequent of an ordinary subjunctive conditional and then denying the whole thing: "It is false that if you were to sleep all morning you would not be tired". The "even if" conditionals must be reduced to this form; hence they would read: "It is false that there is a true statement p . . . etc".[2] (If we reformulate the even-if conditionals in the manner suggested, we may then say correctly that the problem of the subjunctive conditionals concerns the *connection* which obtains between antecedent and consequent.) With all these qualifications, however, we still cannot make the formula sufficiently restrictive.

Suppose that one afternoon two men, quite independently of each other (as we should ordinarily say), were to sit on the same park bench, that they were alone there, and that, as it happened, each of them was Irish. We could then say: "(x) if x is on . . . park bench at . . . time, x is Irish". Our formula is such that, if we were to apply it to this case, we could infer: "If Ivan were to be on . . . park bench at . . . time, Ivan would be Irish". But this conclusion would hardly be warranted. (It would be warranted, if we were to interpret the subjunctive

[1] The term *statement* is so used here that "Frank sees the play" and "The play is seen by Frank" are different statements. Since the formula refers to statements and not to propositions, some such term as Quine's *statement matrix* might be preferable to *propositional function*.

[2] These "even if" subjunctives are usually employed either (*a*) when we have affirmed the consequent and desire to stress its inevitability, or (*b*) as appendages to other subjunctive conditionals (*e.g.* "If you should work like that all night you would be tired and even if you were to sleep all morning you would still be tired").

conditional as saying "If Ivan were *identical* with any one on the bench . . . ", but this, as we shall see, is not what we should ordinarily intend it to say.) Again, consider a small community where each of the lawyers happens to have three children. We may say: "(x) if x is a lawyer in . . . community in 1946, x has three children". But we should not want to say of Jones, whom we know not to be a lawyer there, that if he *were* to have practised there he too would have had three children. The difficulty is that our universal conditionals about the park bench and the lawyers describe what are, in some sense, "accidents" or "coincidences". How are we to distinguish such "accidental" conditionals, of which examples are easily multiplied,[1] from statements such as "all men are mortal", "All wolves are ferocious", etc., which describe "non-accidental" connexions? This is the crux of the whole problem. Our formula must exclude these "accidental" universal conditionals; but the only means we have of distinguishing these is to note that, unlike the "non-accidental" ones, they do not warrant the inference of certain contrary-to-fact conditionals. That is to say, in the case of the park bench we should hesitate to infer "If a were on the park bench, a would be Irish"; but in the case of the wolves we should not hesitate to infer "If a were a wolf, a would be ferocious". (These considerations will become more obvious when we consider, below, the question of the formulation of contrary-to-fact conditionals.)

It is plain that the statements which formulate "natural laws" are a sub-class of the non-accidental universal conditionals. One cannot say, as most philosophers and logicians now incline to do, that a natural law is merely what is expressed in a synthetic universal conditional. We must find the differentia so that we can exclude the "accidental" conditionals. The alternatives are: (1) supply the qualification which our formula lacks and thus reduce the subjunctive to the indicative; (2) accept the subjunctive as describing some kind of irreducible connection and thus reject, or alter radically, the extensional logic which most contemporary logicians have tried to apply to the philosophical problems of science. The problem is not an easy one; indeed, we may be justified in asserting that it constitutes *the* basic problem in the logic of science.

[1] *Cf.* C. H. Langford, review, *Journal of Symbolic Logic*, Vol. 6, No. 2 (June 1941), pp. 67-8. Langford provides here a very clear statement of the present problem. Along with C. I. Lewis, he has been one of the few logicians to recognise explicitly the importance for the logic of science of the subjunctive conditional.

IV

There are three further considerations which will enable us to see more clearly what is involved in this problem.

(1) A contrary-to-fact conditional, when formulated in the customary manner, may give rise to misunderstanding if considered outside the context of its utterance. Given a conditional with an antecedent of the form "if x were y" one may ask whether the supposition is that x is changed to accommodate itself to y or y is changed to accommodate itself to x. I might say, for instance, "If Apollo were a man, he would be mortal", to which the reply could be made, "No: if Apollo were a man, at least one man would be immortal". The possibility of this type of misunderstanding is most apparent where the antecedent of the conditional designates some equivalence relation (*e.g.* "if x were identical with y", "if x were in the same place as y") or some relation of comparison (*e.g.* "x is greater than y"). But theoretically it might occur in connexion with the interpretation of any antecedent.

Let us refer to "If Apollo were a man, he would be mortal" as *a* and to "If Apollo were a man, at least one man would be immortal" as *b*. Knowing Apollo to be immortal and all men to be mortal, should we assert *a* or *b*?[1] The answer depends upon whether we are supposing our beliefs about Apollo, or our beliefs about men, to be contrary-to-fact. (If we were supposing *neither* to be contrary-to-fact, the antecedent would be, not merely false, but contradictory; if we were supposing *both* to be contrary-to-fact, we could assert neither *a* nor *b*.) Ordinarily the context of inquiry determines which supposition is being made. But in a language which was logically adequate, the antecedents of these conditionals would be so formulated that such misunderstanding and ambiguity would not arise.[2] Thus one who had asserted *a* instead of *b* would have said something like,

[1] It was evidently difficulties of this sort which led Broad to question whether subjunctive conditionals about individuals were meaningful if taken literally. (*Op. cit.* Vol. I, pp. 273-278.)

[2] In their customary formulation, the antecedents of subjunctive conditionals are, to a certain extent, analogous to "He is a thief", "I am hot", "Your dog is here", etc., which statements, when considered in isolation, are incomplete and may be true or not, depending upon which of the many possible interpretations of "I", "He", "here", etc. are selected. Like the subjunctive conditionals, these statements are such that when uttered in ordinary discourse, the context of their occurrence determines the interpretation, but in a logically adequate language they would receive a more satisfactory formulation.

"If Apollo were different from what we have believed him to be and had instead the attributes which all men possess, then he would be mortal". And one who had asserted *b* would have said something like, "If the class of men were wider than what we have believed it to be and included Apollo, then some men would be immortal". In the first case, Apollo's status is in question and one is supposing certain commonly accepted statements about him to be false, and in the second case, it is not Apollo, but it is the class of men, which is in question. The advantage of thus formulating the antecedents of these conditionals, so that the wording leaves no doubt concerning which is the object of hypothesis and which is assumed to remain "as is", is further evident when we consider the extent to which the usual canons of inference may be applied to subjunctive conditionals when the object of hypothesis is left ambiguous.[1]

Let us assume, for the moment, the view held by Wittgenstein, Ramsey, and others, according to which, "For all x, fx" is held to be equivalent to the logical product of the values of "fx" (*i.e.* to the conjunction of fx_1, fx_2, fx_3, etc.) and "There exists an x such that fx" is held to be equivalent to their logical sum (*i.e.* to the alteration, either fx_1, or fx_2, or fx_3, etc.). This view, whatever its limitations as an ultimate ontology, has the advantage that it makes clear the manner in which valid inferences can be made connecting particular instances with the general rules under which they fall.[2] Suppose, now, we are considering "If Apollo were a man, Apollo would be mortal". The statement p in our translation may be assumed to be "All men are mortal and there are men". Let us assume that Socrates, Plato, and Aristotle are all the men there are; p then becomes "Socrates, Plato, and Aristotle are men and are mortal". But this statement, taken in conjunction with "Apollo is a man" does not entail "Apollo is mortal". Hence one might contend that use of the contrary-to-fact conditional necessitates the preservation of an "element of generality" in our universal statements, so that "all men" will refer to more than the particular men who will have existed.[3] And it might be concluded, therefore, that

[1] There seems to be a convention implicit in ordinary discourse according to which the antecedent is always so formulated that the subject-term designates the entity which we are supposing to be different or are considering hypothetically. When one says, "If Paoli were the same size as New York . . .", it is more natural to conclude "Paoli would be larger than it is" than "New York would be smaller". The latter conclusion would be drawn from the converse of our antecedent.

[2] *Cf.* Ramsey, *op. cit.* pp. 153-154.

[3] What this reference to an "element of generality" means, of course,

the contrary-to-fact conditional, even granted the adequacy of our formula for translation, presents unique problems in the theory of inference, for we do not encounter such difficulties in connection with "If Socrates is a man, Socrates is mortal".

As in the previous instance, however, the apparent difficulty is explained by the fact that the antecedents of subjunctive conditionals are usually formulated inadequately. The difficulty vanishes if we formulate them in the manner proposed above. If, instead of "If Apollo were a man", we say something like "If Apollo were different from what we have believed him to be and had instead the attributes which all men possess", the problematic inference is seen to be valid, even though statements about all men refer only to Socrates, Plato and Aristotle. Hence, by formulating the antecedents of subjunctive conditionals unambiguously, we cut the ground from under two objections which might otherwise be made to the use of this type of statement.

(2) It is very important to note that, wherever we have a "non-accidental" non-vacuous universal conditional, we can always supply an "accidental" one which will cover the same instances. Suppose, for instance, (i) "(x) if (x) drinks from that well, x is poisoned" is such a conditional. And suppose that, of those who have thus been poisoned, one was born in place p at time t, another in p' at t', etc. We can assert the "accidental" conditional: (ii) "(x) if x is born in p at t, or in p' at t', etc., x is poisoned". It is quite plain that (ii) is accidental and (i) is not, for, given (i) we could infer "If a were to drink from that well a would be poisoned"; but, given (ii), we cannot infer "If a had been born in p at t, a would have been poisoned". Whether a universal conditional is to appear "accidental" or not thus depends upon how one has *described* the entities which fulfil the component clauses.[1] This suggests that the terms of "non-accidental" connexions are the *properties* of things. And if we

is not altogether clear, but, as we shall see, it is the sort of thing which we must countenance if we are to regard the subjunctive as irreducible.

[1] These considerations are unquestionably connected with the distinction, recently noted by Nelson Goodman, between "projectible" and "non-projectible" predicates and with the fact that degree of confirmation "varies widely with the way the given evidence is described". ("A Query on Confirmation", *Journal of Philosophy*, Vol. XLIII, No. 14, pp. 382-5.) The distinction between "accidental" and "non-accidental" universal statements is fundamental to the theory of confirmation. These considerations also suggest the possibility of an alternative formula for eliminating the subjunctive, referring to classes or properties but not to statements. The "class method", however, seems to involve many more difficulties than does the "statement method" and it breaks down at an earlier point.

cannot get rid of the subjunctive by any other means, we can define it in terms of these "connections". To say "(x) if x were ϕ, x would be ψ", would then be to say "ϕ and ψ are *connected*". *Connection* becomes an irreducible ontological category and a source of embarrasment for empiricism. It was this doctrine that C. S. Peirce was defending with his concept of "thirdness".

It is really not clear, of course, what we are trying to convey when we assert that "connection" or "thirdness" is an ultimate ontological category, if we mean to do more than state the problem. And it still may be that the ontology of logical atomism is correct, that, being creatures having a need to rationalise, we have invented these notions of what-might-have-been and what-might-but-won't-be, and that they have no objective significance. But apparently we can't say all the things we want to say in our more serious moments unless we employ them.

(3) Any formula, of the sort which was described in section III above, must presuppose a satisfactory solution to the problem of the designata of statements. Application of our formula for translation must involve in every case a reference to a *statement* and, in many, if not in the majority, of cases, a reference to a statement which has never been uttered, written down, or even conceived. What is the status of a statement which will never have been made? Is it a merely *possible* statement, one which *might* or *could* be made? It might be assumed that, if we attempt to dispense with entities designated by statements, as is sometimes done in logic,[1] we must employ the contrary-to-fact conditional in discussing the statements which have not been made. In this event, we should find ourselves using the contrary-to-fact conditional in the very application of the formula which was designed to eliminate it. If we were to revise our formula so that it would mention facts or states of affairs where it now mentions statements, we should then have to countenance the existence of entities which are merely possible but not actual states of affairs. In this case, instead of defining the merely possible in terms of the subjunctive (as what *would* happen if . . .), we should be following the reverse course, which would have been easy enough at the outset, although not particularly clarifying. If we are to admit the types of case which give rise to these difficulties, there are apparently only two alternatives left to us. The first is to assume that there exist entities which function as designata of sentences; these may be objectivs, propositions, etc. This assumption, of course, is very often made

[1] *Cf.* W. V. Quine, "Ontological Remarks on the Propositional Calculus", MIND, Vol. XLIII (1934), pp. 472-476; *Mathematical Logic*, p. 32.

on other grounds. If we adopt this course, we may substitute for the reference to *statements* in our formula a reference to *propositions* (or whatever entity we had chosen). But if we are deterred by "the obscurity of these alleged entities", we may attempt to extend the term "statement" beyond its normal usage in order to insure that there be an actual entity to answer for every conceivable statement. But how should we describe the semantical properties of these "statements" (which they must have if they are to be statements at all) except by saying that they *would* designate or *would* denote such-and-such things if some interpreter *were* to take account of them? These difficult questions, however, are beyond the scope of the present paper.

II.—THE EXISTENCE OF MATERIAL OBJECTS.

By A. H. Basson.

Philosophers sometimes claim that we can never know for certain that material objects exist. It has often been observed that this claim is *prima facie* false and absurd, and that no one can really believe it to be true. Consequently, it is alleged, the matter is not open to discussion. The real problem is what makes philosophers assert such an absurd proposition. I think this is partly correct, but not wholly so. We are all acquainted with the reasons philosophers give for saying that we can never know for certain that material objects exist. The most puzzling thing, to my mind, is the apparently simple and convincing nature of these reasons, and the exceedingly difficult and unconvincing nature of all attempts to refute them and to establish what is evidently true. After all, one would have thought that the most obvious truths should be susceptible to the clearest and simplest proofs; and that the sophistry necessary to maintain obvious untruths should be correspondingly complicated and obscure, if only to hide away the illogicalities upon which the success of the sophistry depends. Furthermore, if apparently simple and clear arguments which issue in absurd conclusions are to be refuted at all, they must be refuted by arguments at least equally simple and clear. No more complicated argument will suffice, just because we must put our trust in what we see most clearly. In the present case we cannot produce any such simple refutation. No sufficiently simple refutation of the arguments against our knowledge of the existence of material objects *exists*, just because, if it did exist, it would be so obvious that it would not need pointing out.

The easiest approach to this curious situation is to admit that the philosophers' arguments are in fact sound. It is, however, open to us to suggest that these arguments may not be about the things they appear to be about. Consequently, when the philosopher says that we can never know for certain that material objects exist, he may *not* be contradicting the ordinary man when he says that he knows that tables and chairs exist, that tables and chairs are material objects, and consequently that he knows that material objects exist. If this is so, the following question naturally arises: If, when the philosopher talks about knowledge

of the existence of material objects, he does not mean what the ordinary man means when *he* talks about knowledge of the existence of material objects, what *does* the philosopher mean? I propose to consider this question.

Before I embark on the main part of my discussion I should like to mention two distinctions which have frequently recurred in arguments about this problem, and which seem to me either completely irrelevant, or else put in a very misleading way. The two distinctions are closely connected with one another. First, it is said that the dispute about our knowledge of the existence of material objects is a non-empirical as distinct from an empirical dispute. The philosopher, it is alleged, is claiming that it is *logically* impossible or *self-contradictory* to claim knowledge of the existence of material objects. And presumably he infers from this that it is logically impossible or self-contradictory to claim knowledge of the existence of a particular material object, such as St. Paul's Cathedral. This may well be so, the philosopher may be making a purely logical claim, but it by no means entails that the ordinary man is also making a logical claim when he says he can and does know that material objects exist. Indeed, I think it is quite clear that when the ordinary man says he can and does know that material objects exist, that St. Paul's Cathedral exists, that St. Paul's Cathedral is a material object, and, therefore, material objects exist, he conceives himself to be making an empirical *factual* claim. Consequently, it is misleading to say that the *dispute* is non-empirical, and equally misleading to say it is empirical. It is, if anything, quasi-logical, quasi-empirical. It is in fact a *philosophical* dispute.

The second distinction which is often made in discussions of this subject, arises from the first, and I believe it to be completely irrelevant. The distinction between logical and empirical propositions being made and accepted, it is often argued that we *can* be certain of the truth of logical propositions, whereas we *cannot* be certain of the truth of empirical propositions. In the crude form it is alleged that the *notion of certainty* does not apply to empirical propositions. In a more refined form it is alleged that *a* notion of certainty *does* apply to empirical propositions, but that this is different from the notion of certainty which applies to logical propositions. This seems to me to be extremely misleading. The feeling of certainty is the same no matter what the subject may be. Either you are certain or you are not, and there can be no qualification of this. Thus we cannot say that the notion of certainty applied to logical propositions is different from the notion of certainty applied to empirical propositions, simply

because there is only one kind of certainty. And, if by saying that the notion of certainty does not apply to empirical propositions, we mean that it *cannot* be applied to them, we are obviously saying something quite false. The notion of certainty *is* often applied to empirical propositions, in the sense that we do often feel certain about them. The question is whether it is ever *properly* applied in such cases.

This confusion brings to the surface two others. It is now fairly clear that what the philosophers who say we can never know for certain *mean* is not that we can never feel certain, because we often in fact do feel certain, but that we are *never justified* in feeling certain about the truth of empirical propositions, or of the existence of material objects. They wish to add that we *are* sometimes justified in feeling certain about the truth of logical propositions. The other subsidiary confusion rests on the failure to observe this distinction between a feeling of certainty and a *justified* feeling of certainty. This failure may produce a tendency to *define* what we feel certain about as analytic, and to define what we doubt as empirical. If we do this it is quite clear that we can never know for certain any empirical fact. This cannot be denied; we can only observe that it would provide a very unusual definition of the domain of logic.

It is now safe to say that what the philosopher means when he says we can never know for certain that material objects exist, is that we can never be *justified* in believing that material objects exist, and consequently we *are* never justified in saying that particular material objects, St. Paul's Cathedral, for example, exist. The ordinary man says we *are* sometimes justified in believing that particular material objects, such as St. Paul's Cathedral exist, and consequently we must be justified in believing that material objects exist. The question then arises: what can the philosopher possibly mean by saying that we can never *be justified* in believing that material objects exist? What *is* the justification of a belief?

In order to answer this question we must first point out that there are two completely different senses of the phrase 'justified belief'. These two senses are respectively connected with what I shall call 'true beliefs' as distinct from 'knowledge in the strict sense'. I have to introduce these rather unusual phrases because the distinction I have in mind is not clearly made in ordinary speech. It is brought out in the following way: If we ask someone 'Do you know if there is cheese in the cupboard?' and he says 'Yes', we may then ask 'But do you *really know?*' and he may perhaps hesitate and say 'Well, I saw something that

looked like cheese'. In this way we demonstrate that, although he may not have doubted there was cheese, and although there may have *been* cheese, he did not *really know* there was cheese in the sense of having *sufficient evidence* that there was cheese. Thus you may know that St. Paul's Cathedral exists, in the sense of believing that it exists, and not doubting that it exists, and it does in fact exist; and yet at the same time you may not know in the strict sense that it exists, that is, you may not have *sufficient evidence* that it exists.

Now I do not think the philosophers wish to deny that we know that material objects exist, in the sense of entertaining a true belief that material objects exist. They could not do so unless they maintained that we do know that material objects do *not* exist, and they cannot say this because they wish to say that we can know that material objects *probably* exist. Consequently, the philosophers do not wish to contradict the ordinary man when he says he knows that material objects exist, meaning by this that he sometimes entertains true beliefs about the existence of material objects. The ordinary man would, however, say that he not only entertained true beliefs, but also that he sometimes has sufficient evidence to justify them. So there is still an apparent difference between the ordinary man and the philosopher, and it will be necessary to enquire what the philosopher means by 'sufficient evidence to justify a belief'.

The problem will be clarified if we can decide whether the philosophers who say we can never have sufficient evidence for the existence of material objects, wish also to say that we can never have sufficient evidence *against* their existence. They hesitate a little on this point, but on the whole they seem to wish to say both. They want to say *both* that we are never justified in feeling certain that the table before us exists, *and* that we are never justified in claiming to know that the drunkard's pink rats do not exist. They want to say *both* that we can never have sufficient evidence for the table, *and* that we can never have sufficient evidence against the rats. It remains theoretically possible, however, to say *either* that we can never have sufficient evidence for the table, although we can have sufficient evidence against the rats, *or* that we can have sufficient evidence for the table, although we can never have sufficient evidence against the rats. I mention these alternatives because some philosophers do sometimes speak as if they held one or other of them.

It is now safe to ask: What is 'sufficient evidence'? What do the philosophers mean by this phrase? One claim which is often implied, although it is seldom openly made, is that 'sufficient

evidence for' means '*all* the relevant evidence, and that *all* this is for and *none* against'. Similarly, 'sufficient evidence against' is held to mean 'all the relevant evidence, and that all this is against and none for'. Now it is plain to everyone that very few propositions whose evidence has been examined, have even all the *available* evidence for and none against. And no claim which has ever been made has all the available evidence against and none for, simply because the fact that the claim has been made is *prima facie* evidence for it. Many propositions like 'St. Paul's Cathedral exists' have some evidence against them, even if the vast weight of evidence is favourable; and *all* propositions which deny antecedent assertions, such as 'The pink rats he thinks he sees do not exist', have some evidence against them, even if the vast weight is in their favour.

Consequently, if we cannot know that the table before us exists unless we have all the relevant evidence and all this is favourable, and if we have some evidence, and some of this is favourable and some not, then it is quite clear that we can never know that the table before us exists, even though we gather evidence through all eternity. It is equally clear that this is a misleading way of putting the matter. What we do know in cases like this, and on these assumptions, is that it is wrong, although it may not be false, to say that the table does exist, *and* that it is wrong, although it may be true, to say it does not exist. It is wrong, and always will be wrong, *and can never be right* to say either that the table does exist or that it does not exist. The puzzle is then to say what we can possibly mean by saying that the one may still be true and the other false, although it is wrong and always will be wrong, and will never be right, to say either.

This awkward position can be avoided be defining 'sufficient evidence' in a more common-sense way. 'Having sufficient evidence for' means, not 'having all the relevant evidence, and all of it being for', but rather 'having all the relevant evidence, and most of it being for' or 'having most of the relevant evidence, and all of that being for'. I think philosophers often mean one or both of the latter, although they do not clearly distinguish them from the former. When they say we can never have sufficient evidence that the table before us exists, they want to suggest, not only that we cannot have all the evidence but also that we cannot have most of the evidence. They want to suggest that we can never have a *decisive weight* of evidence. The first thing that strikes us about this suggestion is that it is clearly false. We certainly do have most of the evidence, a

decisive weight of evidence, about this table, and all we have is favourable, and so in the sense defined we really do know that this table exists. The philosopher does nothing but flatly deny this, and since what he says is plainly absurd and contrary to common sense and since what we say is obviously true and sensible, there seems no reason whatever to agree with him.

There is, however, a way out of this for the philosopher. He may say something like this: If this table exists, its existence will have a train of consequences of indefinite length, and the occurrence or non-occurrence of each and every one of these consequences will constitute evidence for or against the existence of this table. There will be more evidence available to us tomorrow, and more the day after, and if we are alive in fifty years time there will be more still, and if anybody is alive in a thousand years time there will be more evidence available to them. And maybe we live on eternally, and if we do we shall be in a better and better position to judge about the table as time goes on. But we shall never be in a position to make a final judgment, nor will anyone else, just because there is no end to the chain of consequences.

This is a very queer argument because it turns all our ideas about knowledge upside down. Surely the very opposite of what the philosophers say is true. We are in a better position to judge about this table here and now than we shall be in fifty years' time, when those of us who are left will probably have forgotten all about the matter. We, here and now, are certainly in a better position to judge than the historians of a thousand years' time. You might as well say that we are now in a better position to judge the existence of the Round Table, than were King Arthur and the Knights who sat round it. Thus, what is most interesting about the philosophers' argument is not its validity, but the specious air of plausibility which surrounds it. I think there are two reasons for this.

The first thing which lends plausibility to the philosophers' argument arises from the plain fact that we are sometimes in a better position to judge the existence of the drunkard's pink rats than the drunkard is himself. This makes us realise that sometimes other people may be in a better position to judge than we are, and sometimes we may later on find ourselves in a better position to judge than formerly. This makes us tend to say, quite falsely, that there is *no* best position, that some are just better than others. We tend to ignore the plain facts that there *must be* a best position, and that sometimes we occupy it, and when we do we are in a position to know, *really* to know.

The second thing is the scientific feeling we all have that all things are bound together by a mesh of necessary causal connections. This makes us feel that, if the table before us exists, then certain things will happen in a thousand years, which will not happen if it does not now exist. And we feel that the existence of this table would have an indefinite chain of consequences. Here we ignore the fact, that, *if* the present existence of this table has a chain of consequences of indefinite length, then the *value* of the consequences as evidence for the present existence of this table will become smaller and smaller as the chain lengthens. In the remote future, when the chain becomes very long indeed, the *value* of the consequences as evidence for the present existence of this table will become vanishingly small. In other words, although the evidence may be infinite in *bulk* as it were, it is not infinite in weight. And the weight of the evidence is not evenly distributed through the bulk, but tends to be concentrated in the evidence available at the time when the claim is made. If you like to put it another way, the increasing bulk of evidence is represented by a divergent series, but the increasing weight of evidence is represented by a convergent series. In this way, although the bulk of the evidence may be infinite, and although the evidence we have can only be of finite bulk, we may yet have most of the *weight* of evidence. If we have most of the weight of evidence, we are clearly *justified* in claiming knowledge of the existence of the table before us.

There is, however, one other way out for the philosopher, which I now propose to consider. He may say something like this: I agree with all you have said, except for the conclusion. I agree we may sometimes have most of the evidence in the way you describe, and if this is what we mean by 'know', then we can know things in the way you describe. We can know there is a table before us, both in the sense of truly believing, and in the sense of having sufficient evidence for our belief. Nevertheless, I still maintain that we cannot ever really know there is a table before us. What I wish to say now, and perhaps what I meant all along, is this: When I say we can never really know, or we are never justified in being certain, I mean, *not* simply that we can never have sufficient evidence, but that we are never justified in feeling certain that the evidence we have *is* sufficient evidence. In order to avoid a vicious regress, which would involve both sides in a stalemate, I am prepared to agree that the sufficiency of evidence may ultimately be *self-evident*. Thus the real dispute is not a dispute about the sufficiency of evidence at all.

Let me try to make this a little more clear. The philosopher

wants to say that, although we may sometimes have sufficient evidence, and although the sufficiency of this evidence may be self-evident, nevertheless we are never justified in claiming that it *is* self-evident. He wants to say this because he knows, and knows we agree, that mistakes sometimes occur; and mistakes could not occur if the *insufficiency* of evidence were also self-evident. He wants to infer from this that we cannot ever really know there is a table before us, because we cannot ever decide whether the evidence we have is self-evidently sufficient, or whether it is not sufficient, although this is not self-evident. Since evidence sometimes appears to be sufficient when it is not, we cannot ever really know whether the evidence we have is *really* sufficient, or whether it just appears to be sufficient although it is not really.

The argument can be put more simply. It is premised that (1) we sometimes make mistakes, and (2) the essence of a mistake is that we cannot know at the time that it *is* a mistake (it looks just like the real thing). From this it is argued that, (1) we cannot ever perceive the difference between a mistake and the real thing, and consequently that (2) we cannot ever perceive the difference between the real thing and a mistake.

Put in this way, the argument is seen to be invalid. The fact that we cannot know at the time that we are mistaken, does not entail that we cannot know, at the time when we are not making a mistake, *that* we are not making a mistake. The point I wish to draw out here is that we always need evidence that our judgement was mistaken, we do need evidence that what we said was false; but we do *not* always need evidence that our present judgment is not mistaken, we do not need further evidence that what we say is true.

In spite of this a further attack may yet be made, and I think the answer to it will bring out the point I have in mind. The argument goes as follows: If, when you say you know there is a table before us, you mean you have sufficient evidence and its sufficiency is self-evident, then you mean that nothing which could possibly happen in the future will lead you to deny, or even to doubt, that there *was* a table here before us. In this case you are saying nothing of any value when you say that there is a table here before us, and you certainly do not mean what people usually mean by that statement. If by making the statement you do mean to say something about the future, then what happens in the future will constitute evidence for or against it. You cannot have now the evidence you will have in the future. If, therefore, by making this statement about the present, you

mean to imply something about the future, the evidence you have must *necessarily* be insufficient. If you do not, your statement conveys no meaning to anyone, except perhaps in so far as it is a report of your present mental state. Consequently, *either* you cannot really know what you claim to know, *or else* your statement conveys no information.

We wish to claim, of course, *both* that we sometimes know, really know, matters of fact, *and* that our statements about them provide information. The objection is, therefore, a serious one. Nevertheless I believe it to be invalid, and I will try to show why.

It is assumed in the argument that there is a necessary connexion between the presence of the table here and now, and the occurrence of some events in the future. It is assumed that the present existence of the table, or rather *assertion* of its present existence, entails assertion of some future existence. Now we may easily see that the presence of the table here and now, does *not* entail its presence in a month's, a week's, or a day's time. The presence of the table here and now *does not* entail its presence in even a moment's time because this would entail its presence through all eternity. The presence of the table here and now renders it *highly probable* that it will be here in an hour's time, and very highly probable indeed that it will be here in a moment's time. But it does not *entail:* there is no *necessary connexion* between its presence here and now and its presence in a moment's time. Similarly, the presence of the table here and now does not entail, but renders it highly probable, that it was here a moment ago. Now, suppose the table suddenly disappears. Are we to say that the fact that it was here before makes it highly probable that it is here now, although it does not appear to be? Or are we to say that the fact that it is not here now makes it highly probable that it was not here before, although it seemed to be? Obviously we cannot say both. If each casts doubt on the other, neither is possible. Thus it is *doubt* that needs justification, not certainty. We are doubtful of some things because we are certain of others, and not *vice versa.*

We can now consider the bearing of this on the question of conflicting evidence. We want to say that the evidence we have makes it *certain* that the table is here now, and that this makes it *highly probable* that we shall have other 'evidence' in the future. Now suppose the unexpected happens, and the table suddenly disappears. What is the bearing of this new evidence? *If* the evidence we have is sufficient to make it *certain* that the table is not here now, this makes it *highly improbable* that the table was there before, and this makes it *highly improbable* that the evidence

we believed to be sufficient *was* sufficient. But we cannot argue from the improbability of the sufficiency to the probability of the insufficiency of our later evidence; because this would make the present non-existence, and consequently the past non-existence, of the table *less* probable; and this would make the insufficiency of the former evidence *less* probable, and this would make the insufficiency of the later evidence *less* probable than we had assumed. Consequently, whatever degree of probability of insufficiency of the later evidence you choose to infer from the improbability of the sufficiency of the earlier, you can prove it is in fact less. Hence it is self-contradictory to infer from doubt of the past existence of the table to doubt of its present non-existence. And it is equally self-contradictory to infer the other way.

I think it is clear from this, that, not only does doubt of material facts rest on certainty of some others, but also that doubt of the sufficiency of evidence likewise rests on certainty of the sufficiency of other evidence. Hence, we can know, really know, matters of fact, and that material objects, tables, chairs and the like do really exist.

The philosophical dispute seems to arise mainly from two confusions. The first is the tendency to regard doubt as proceeding from too little evidence simply. Doubt is thus regarded as the initial state, as it were, and certainty as something requiring justification, and it is thought that, if the justification is not forthcoming, we 'ought' to doubt. I suspect that statements like 'the notion of certainty is not applicable to empirical propositions' and 'the notion of certainty as applied to empirical propositions is different from the notion of certainty as applied to logical propositions' are nothing but misleading ways of saying we ought always to doubt empirical propositions. Indeed, one tends to suspect that these apparent statements are really cloaked imperatives, quite lacking any assertive import or foundation. In point of fact, the very opposite is the case. Doubt arises from evidence which is both too much and too little, too much to allow us to say the table is not here, and too little to allow us to say it *is* here. Doubt is in fact derivative, and certainty ultimate. The confusion arises from falsely regarding doubt as the fundamental thing and then perceiving that certainty cannot rest on doubt, a necessary but somewhat misleading truth.

The other cause of confusion is due to the tendency to regard material existences as *entailing* specific future experience. As every sensible person agrees that we cannot know beforehand *exactly* what our future experiences will be, this suggests that we

cannot ever know what material objects exist, if any. Here there is a subsidiary confusion between saying we can never know *exactly* what our future experiences will be, and saying we can never know, really know for sure, what our future experiences will be, more or less. These two quite different propositions are fused into: we can never know for sure exactly what our future experiences will be. Everyone agrees about this, but it does not entail either that we can never know for sure what our future experiences will be, more or less, or that we can never know exactly what our future experiences will probably be. We can and do know both these things.

In conclusion, I should like to revert for a moment to the distinction between empirical and analytic certainty, which has so confused the issue in discussions of this subject. I fail to see what can be meant by asserting that we can be sure of analytic truths, if it is granted that we cannot be sure of empirical truths. Surely it cannot be suggested that errors never occur in logic and mathematics, or that no logical propositions require evidence, or that no dispute ever arises about the sufficiency of the evidence for logical propositions. Surely it was adequately demonstrated by Hume that, if we can never be sure of empirical truths, we can never be sure of analytic truths either. And if this is so, the whole argument about uncertainty is itself uncertain. I think people are misled into making this distinction simply because the fundamental nature of certainty is so much more obvious in logic and mathematics than it is elsewhere. It is so much more clear in these subjects that the fundamental certainties cannot be supported by evidence simply because they do not require any, and they do not require any because they have none.

III.—CONTRADICTION AND THE PRESUPPOSITION OF EXISTENCE.[1]

By Everett J. Nelson.

1. Introduction.

I wish to discuss an issue which was the subject of a controversy many years ago between Langford and Chadwick.[2] Chadwick argued for and Langford against the view that a singular proposition has a contradictory. In my opinion, Langford successfully met Chadwick's arguments. Because of lack of time, I shall omit further reference to their disputation, except to indicate Langford's position. He held not only that $\sim fa$ is not the contradictory of fa because both of them imply that an individual exists, and so are existential, but also that fa does not have a proper contradictory at all.

It is due to my reluctance to accept this conclusion and its consequences, that I reopen the discussion. First, I shall restate the problem; second, I shall extend it to properties or predicates, pointing out certain consequences of current logic on the issue before us and on a more general issue of which this is a special case; third, I shall suggest for your consideration a distinction between what a proposition asserts and what it presupposes, in terms of which we may overcome the apparent inconsistency between our demand that propositions have contradictories and the alleged existential import of some, if not all, propositions. And finally, I shall suggest how the connexion required for the inference of particular propositions from singular ones may be retained in spite of its apparent disruption by our distinction.

2. The Problem

(1.1) The problem before us arises from the allegation that both the singular proposition fa entails the general proposition $(\exists x)fx$ and also the singular proposition $\sim fa$ entails similarly the proposition $(\exists x) \sim fx$. As a consequence thereof, both fa and $\sim fa$ entail $(\exists x) . fx \vee \sim fx$, or that an individual exists. Now, if this existential consequence common to them were false, both

[1] A slightly revised version of a paper read at the meeting of the Pacific Division of the American Philosophical Association at the University of California, December, 1945.

[2] Mind, Nos. 143, 145, 148 and 150.

of them would be false. Consequently, they are not contradictories.

(1.2) The same problem might seem to arise also from the commonly accepted doctrine that fa and also $\sim fa$ entail that a exists, because a is a constituent of both of them. Thus, if fa were true, and even if it were false, the individual a would have to exist: the very being of either of the propositions fa or $\sim fa$ involves the existence of a. Accordingly, since fa and $\sim fa$ entail "a exists", and since it is not logically certifiable that anything exists, it would seem that both of them could be false.

There are, however, difficulties in this position, because there seem to be compelling reasons for believing not only that "a exists" can be false, but also that it cannot be false. (1) "a exists" can be false, for it implies $(\exists x) . fx \vee \sim fx$, or that something exists, which surely is not certifiable on the grounds of logic alone, and so is contingent and could be false. (2) Yet, since "a exists" has the individual a as logical subject, it could not fail to be true. I believe that the contradiction between (1) and (2) is more apparent than real. We can indeed know *a priori* that any value of "x exists" is true because this function is so constructed that any value of it will contain as a constituent a sufficient ground of its truth. Nonetheless, I should say that "a exists" is a contingent proposition because its very being depends upon contingent fact and because *it* cannot be known, or even entertained, independently of direct experience with the individual or particular a. There seem then to be distinct types of propositions which are necessary and can be known *a priori*; namely, (1) those which are structurally tautological; *e.g.* $(x) . fx \vee \sim fx$; (2) those not structurally tautological but which contain a sufficient ground of their truth; *e.g.* "a exists";[1] and (3) those having the properties of both (1) and (2); *e.g.* $fa \vee \sim fa$. Propositions of the second kind have the peculiarity that though they are necessary and *a priori*, still they have consequences which are neither necessary nor *a priori*, either tautologically or contentually.[2]

Accordingly, the situation seems to be this: We cannot argue that both fa and $\sim fa$ can be false because they imply "a exists", though we can argue this on the ground that, since implication

[1] We could include here also such impossible propositions as "a does not exist", which contain a sufficient ground of their falsity.

[2] Employing a distinction to be drawn in Section 3, I should explain the necessity of propositions of the second type on the ground that they "presuppose" themselves. Hence, we have here a ground for necessity different from "tautology".

is transitive and since "a exists" implies $(\exists x) . fx \vee \sim fx$, fa and $\sim fa$ severally imply this latter proposition which could be false. This however, is the point of the preceding sub-section; hence the common implication of fa and $\sim fa$ that "a exists" is not really an alternative way of raising the same problem.

(2) Granting that the foregoing existential consequence, $(\exists x) . fx \vee \sim fx$, was validly drawn from fa and from $\sim fa$, we may, by employing analogous reasoning, disclose another aspect of our problem by deriving further consequences of fa and $\sim fa$, which seem to have been overlooked. *If* fa be existential as to its individual constituent a, then surely it is likewise existential with respect to its predicate or property constituent f. That is, if fa entails that a exists, then for a similar reason it entails that f exists (or subsists or has whatever ontological status it actually does have.)[1] *E.g.* if "a is red" is existential because it contains the constituent a, then it is existential because it contains the constituent *red*. Moreover, if fa entails $(\exists x)fx$, then for like reasons it entails $(\exists P)Pa$, *i.e.* that there is a predicate P such that a has it.[2] Corresponding consequences follow from fa; to wit, f exists, and $(\exists P)Pa$. So both fa and $\sim fa$ have the common consequences, f exists, and $(\exists P) . Pa \vee \sim Pa$, besides those set forth a few moments ago.[3] And just as the existence of individuals is contingent, so the existence of many properties is not certifiable by formal logic. Hence, it would follow that fa and $\sim fa$ are not contradictories, not only because they are existential as to individuals but also because they are existential as to properties.[4]

In fact, the entailment of the existence of properties has a wider range of application than that of individuals, because universal propositions, even if not existential as to individuals, as some of us hold, would nonetheless be existential as to properties, since they have properties as constituents. Thus $(x)fx$ and its presumed contradictory $(\exists x) \sim fx$ entail in common $(\exists P)P = f$ and so are not contradictories.[5]

[1] For sake of simplicity and avoidance of unnecessary verbiage, I shall speak of properties as existing, realising that there is a difference between the status of individuals and that of properties.

[2] Still other consequences could be mentioned; *e.g.*, $(\exists P)P = f$, and $(\exists x)x = a$, etc.

[3] Like "a exists", "f exists" is *a priori* and contentually necessary.

[4] Even if it should be shown that such existential propositions cannot be false, still I should maintain that no two propositions are contradictories if they have a common implicate, regardless of the status of that implicate. This point will be considered in Section 4.

[5] If we follow the material logicians in holding that universal propositions are existential as to individuals also, then we may exhibit the

(3) Before turning from common doctrine and its consequences, I want to consider in a somewhat different manner the proposition fa implies $(\exists x)fx$, not only because this proposition is, as far as I know, generally accepted, but also because it plays the very important rôle of a bridge between the theory of elementary propositions and the theory of first order propositions. Using currently accepted material principles, I am going to derive for consideration a consequence of this proposition.

Granting for sake of argument that fa implies $(\exists x)fx$, it follows that

$$fa : \equiv : fa \,.\, (\exists x)fx \quad . \quad . \quad . \quad . \quad (1)$$

Since, if any proposition p is equivalent to q, then $\sim p$ is equivalent to $\sim q$,

$$\sim fa : \equiv : \sim fa \,.\, \vee \,.\, (x) \sim fx \quad . \quad . \quad (2)$$

Also, for the reason justifying (1), we have:

$$\sim fa : \equiv : \sim fa \,.\, (\exists x) \sim fx \quad . \quad . \quad . \quad (3)$$

Replacing the left side of (3) by its equivalent as given in (2), we get

$$\sim fa \,.\, \vee \,.\, (x) \sim fx : \equiv : \sim fa \,.\, (\exists x) \sim fx \,. \quad . \quad (4)$$

Now, addressing those who hold that the contradictory of an existential proposition is not existential, or that a universal proposition is not existential, I shall formulate a condition under which the left side would be true but the right side false; namely, that there are no individuals. $(x) \sim fx$, being equivalent to $\sim (\exists x)fx$, would then be true, whereas $(\exists x) \,.\, \sim fx$, and therefore the conjunction of which it is a member, would be false.

I addressed this argument to a special group, because the material logicians who hold that first order universal propositions such as $(x)fx$ imply existence, would hold that our condition would falsify not just one side but both sides of (4).[1] Accordingly, we should not have supplied them with a reason for rejecting the case of implication under examination.

The upshot of this particular argument is this: Consistency demands that if we accept the premises, namely, that fa and $\sim fa$

existential nature of $(x)fx$ and $(\exists x) \sim fx$ by pointing out that both of them imply $(\exists P)\,(\exists x) \,.\, Px \vee \sim Px$. (Proof: $(x)fx$ implies $(\exists P)\,(x)Px$, which implies $(\exists P)(\exists x)Px$, which in turn implies $(\exists P)\,(\exists x) \,.\, Px \vee \sim P$. Hence, the first implies the last. Similarly, $(\exists x) \sim fx$ implies $(\exists P)\,(\exists x) \sim Px$, which implies $(\exists P)\,(\exists x) \,.\, \sim Px \vee Px$. Hence both $(x)fx$ and $(\exists x) \sim fx$ imply $(\exists P)\,(\exists x) \,.\, Px \vee \sim Px$.)

[1] They would hold also that the condition is logically false, for they hold that one of the basic postulates of logic is that there are individuals.

severally imply $(\exists x) . fx \vee \sim fx$, then we must accept the consequences not only that universal propositions such as $(x)fx$ are existential, but also that they do not have contradictories; or, transpositively, if we want our universal propositions to be non-existential or to have contradictories, we must repudiate the proposition that fa implies $(\exists x)fx$ or that $\sim fa$ implies $(\exists x) \sim fx$.

3. The Distinction

(1) We are faced then with an apparent contradiction: Surely fa and $\sim fa$ are contradictories; but since each entails $(\exists x) . fx \vee \sim fx$, both could be false; hence they are not contradictories. Mindful of the Scholastic precept to draw a distinction when threatened with a contradiction, I shall begin by distinguishing between (1) the constituents of a proposition, and (2) what is asserted about them. Thus, f and a are constituents of fa; and what is asserted is that f characterises a. Now, relative to the proposition of which f and a are constituents, there is an important difference in the statuses of the terms of this distinction; namely, the existence of the constituents of fa is necessary to the very *being* of the proposition fa; whereas the characterisation of a by f is not necessary to the being of the proposition but only to its *truth*. In other words, unless there were the individual a, there could not be a proposition having a as a constituent; hence, a necessary condition of the existence of fa is the existence of the individual a. In like manner, the existence of f is another necessary condition of the existence of fa. We may express these facts by saying that a proposition fa is existentially-dependent upon the being of its constituents, but not upon a having f.

(2) We have distinguished between (1) the necessary conditions of the *existence* of a proposition, and (2) the necessary conditions of its *truth* (exclusive of its existence). Now the distinction I suggest we use in solving our problem is between (1) propositions asserting only necessary conditions of the existence of a proposition, and (2) propositions asserting necessary conditions of the truth of the proposition exclusive of propositions asserting its existence-conditions. Propositions of the former type—*i.e.* those asserting existence-conditions only—I shall call the *presuppositions* of the given proposition; and the propositions of the latter type—*i.e.* those asserting truth-conditions exclusively—I shall call the *assertions* or truth-conditions of the given proposition.[1]

[1] A "truth-condition" of p, as we use the term, does not assert existence-conditions or any conditions upon which both p and $\sim p$ depend.

Accordingly, since *fa* is existentially dependent upon the existence of *a*, *fa* presupposes the proposition "*a* exists", and, consequently, it presupposes also the proposition that some individual exists. Similarly, *fa* presupposes the propositions that *f* exists and that some property exists. Thus, the propositions that there is an individual and that there is a property are presupposed by *fa*. But neither of them is *asserted* by (*i.e.* is a truth-condition of) *fa*.

Before employing this distinction in solving the problem before us, I want to call attention to certain general considerations which support it.

The distinction between what a proposition is about and what it asserts of that which it is about would, I suppose, be rejected by no one. And I doubt too that anyone would hold that part of what a proposition *asserts* is the necessary conditions of the *existence* of what it is about, or is the intrinsic properties of what it is about. Thus, "*a* is red" does not assert, *e.g.* that *a*, being an individual, has space-time co-ordinates; or that red is a concept or an eternal essence or has whatever ontological status it actually does have. Further, I do not believe that part of what a proposition asserts is the necessary conditions of the existence of that proposition. For example, though a necessary condition of the existence of the proposition "*a* is red" is that *a* exists, or that there is an existent thing, still the assertion that *a* exists is no part of the assertion about *a* to the effect that it is red. Similarly, no part of the assertion about *a* to the effect that it is red is the assertion that red exists or, as some would say, subsists. Also, no part of the assertion that "*a* is red" is that there exists a proposition, though that too would be the case. Consider the proposition, "It is false that *a* exists". Although, since *a* is an individual, this proposition is indeed false, it is not false because of any incompatibility between its assertions, for of the two propositions "*a* exists" and "*a* does not exist" it *asserts* only the latter. The reason it is false is that what it does assert is incompatible with one of its presuppositions or existence-conditions, namely, "*a* exists".

I am not concerned to deny that "*a* exists" is in some sense a consequence of "It is false that *a* exists" or of any proposition existentially dependent on *a*. What I am concerned with, is that it is not a consequence of such propositions in the sense in which a truth-condition of them is. In the last section of this paper, I shall indicate the nature of this type of consequence. My point here has been to disclose a straight-forward and im-

portant sense in which I can *assert* fa without asserting that a exists, or that f exists.

Perhaps at this point I should say that in using the word "presupposition" I do not wish to suggest that the meaning in which I have employed it is *the* correct meaning of that word as it occurs in discourse. I do think, however, that that meaning is one of several which it carries.

4. Application of the Distinction

Now I wish to suggest how our distinction between presupposition and assertion will resolve the contradiction which confronted us.

First, since we believe that contradiction is concerned with the truth-conditions of propositions and not with their existence-conditions, we define the relation of contradiction between propositions in terms of what they *assert* excluding what they presuppose. Accordingly, two propositions are contradictories if and only if not both of them can be true and not both of them can be false *because* of relations between their assertions (or truth-conditions) irrespective of their presuppositions (or existence-conditions).

Second, we apply this notion of contradiction to the case of fa and $\sim fa$. Since unquestionably not both of them can be true, the issue turns on whether the truth-conditions of both of them can be false. Concordant with our distinction, neither of them asserts (*i.e.* has the truth-condition) that an individual or a property exists. fa asserts only that f characterises a, and $\sim fa$ asserts only that f does not characterise a. Therefore, since one of the two denies precisely what the other affirms, not both of them can be false. Therefore, fa and $\sim fa$ are contradictories. Thus we have removed the contradiction without having to make an exception to the usual principle that every proposition has a proper contradictory. Further, we have made it possible to deny a proposition having as constituents either individuals or properties without asserting any part of what that proposition asserts.

In order to emphaise that the significance of this definition of contradiction goes far beyond the case of singular propositions, I want to point out that unless contradiction be defined in terms of assertion only, or some other basic alteration be made, not only would singular propositions not have contradictories but elementary functions of singular propositions and general propositions too could not be contradicted. Thus, even such

functions as $p \vee \sim p$ would be existential and hence contingent rather than logically necessary. And, as I indicated earlier, quantified propositions such as $(x)fx$ and $(\exists x) \sim fx$ would not be contradictories because both would be existential as to predicates even if not also as to individuals.

Since what a proposition *presupposes* and also what it *asserts* are usually said to be implied by the proposition, I wish to digress for a moment to remark on implication.

In virtue of our distinction, the implications of a proposition divide into two distinct classes. Consider fa and $\sim fa$ again. According to current doctrine, the class of implicates of fa and the class of implicates of $\sim fa$ overlap, giving three sets of propositions: (α) Those implied by fa but not by $\sim fa$, which we have called the *assertions* or truth-conditions of fa: (β) those implied both by fa and by $\sim fa$, which we call the *presuppositions* of fa and of $\sim fa$; and (γ) those implied by $\sim fa$ but not by fa, which are the assertions of $\sim fa$.

Now, though I do not want to quarrel over the use of a word or raise the question of the analysis of implication, I do wish to suggest that, if we want implication and contradiction to be related in the comparatively simple manner most of us believe them to be related—*e.g.* so that a proposition and its contradictory have no common implicates—we restrict the term "implicate" to what I have called "assertion". Accordingly, we should not say that fa implies that a exists but that it presupposes it, though it does imply that f characterises a. I doubt that such a restriction upon "implication", or that the use of "presupposition" to cover certain cases excluded from the application of "implication", would constitute a violation of good linguistic usage, though it would indeed be at variance with current use in symbolic logic.

5. Reconstruction

Even if it be granted that our distinction has been useful in solving the problem of this paper—namely, that of the contradiction of singular propositions—still it may be felt that the price in principles belonging to a very successful and generally accepted formal logic is too great, for would not our solution invalidate a considerable portion of current doctrine and complicate what is left? It is indeed true that we should have to distinguish between principles involving presupposition and those involving assertion, and we should have to implement the distinction by introducing principles of assertion, principles of presupposition,

and rules relating them. Since such a reconstruction goes beyond the scope of this paper and since it has not been worked out, I shall conclude with a tentative suggestion, limited to one proposition, as to how it might be carried out without sacrificing anything sound in current doctrine.

If implication be restricted to assertion, then the proposition, "fa implies $(\exists x)fx$", would have to be rejected; but passage from fa to $(\exists x)fx$ could nevertheless be validated. In the first place, as we have argued, the relation of fa to $(\exists x)fx$ is not assertion, for fa asserts only that a has f. And in the second place, it is not presupposition either, because, though fa presupposes that f and a exist, it does not presuppose that anything, even a, has the property f. However, $(\exists x)fx$ follows from (1) the presuppositions of fa that there is the individual a and that there is the property f; and (2) the assertion of fa that f characterises a; but not from either alone. For the relation between fa and $(\exists x)fx$, which is generated by these two relations, we might use the term "yield"; thus, fa yields $(\exists x)fx$.

Finally, it is interesting to notice that both presupposing and yielding, like implying, would be bases for inference. Hence, there would be three different grounds for inferring a proposition q, to-wit:

(1) p implies q, and p is true;
(2) p yields q, and p is true; and
(3) p presupposes q (regardless of the truth-value of p).

IV.—ANALYSIS OF "CORRECT" LANGUAGE.

By Yehoshua Bar-Hillel.

The purport of the following criticism of a recent article by Prof. G. E. Moore,[1] is to indicate the essential fruitlessness of the so-called "analytic method", as it is practised by a great many of contemporary British philosophers, especially of the Cambridge School.

I was able to find in this article, invented and (presumably) written by one of the most scrupulous and minute thinkers of our time, several mistakes, ranging from slight errors to quite serious and important blunders, but even the slightest of them of greater weight than that made by Bertrand Russell, to the discussion of which Prof. Moore dedicated almost two pages[2] of his contribution. This fact leads me to assume that there is something fundamentally wrong in Prof. Moore's approach. I shall return to this topic at the end of this article.

(1) I doubt whether Prof. Moore's statement (p. 184) "The assertion, "The sentence, 'The sun is larger than the moon' means neither more nor less than that the moon is smaller than the sun" is certainly true, and yet anybody who asserted it would certainly not be *giving a definition* of the English sentence named", is certainly true and even if it is true at all. I can imagine an English class for foreigners, where the teacher might use just this English sentence to introduce to his class the word 'larger' which had not been taught before. In this case, I believe it would be "good" usage to call this introduction 'giving a definition', because this sense of 'definition' as 'introduction of a new term or a new combination of terms' seems to me to be as "correct" as the sense of 'definition' as 'explanation (or explication or clarification or analysis) of a (perhaps only partially) known expression'. (I would even propose to restrict, by convention, the usage of 'definition' to the former sense only, and use, say, 'explication' for the second sense, at

[1] Russell's "Theory of Descriptions", *The Philosophy of Bertrand Russell*, The Library of Living Philosophers, vol. v (1944), pp. 177-225.

[2] *Op. cit.* pp. 188-189. The point was that Russell used the word 'wrote' in his set of three propositions whose conjunction was to be equivalent to the "author of *Waverley* was Scotch", whereas the correct word should have been 'invented', as one might be an "author" of a literary composition without "writing" it.

least in philosophical articles—a lot of confusion might thus be saved.)

(2) It seems to me that Prof. Moore had not seen far enough when he stated (p. 197) "it is, so far as I can see, a sufficient condition for saying that, in making an assertion of the form "*s* means neither more nor less than *p*", one has *given a definition* (correct or incorrect) of *s*, that the sentence used to express *p* should (1) mention separately a greater total number of conceptions and objects than *s* does, and (2) should also not contain as a part of itself either *s* or any other sentence which has the same meaning as *s*".

According to this statement, in making every one of the following assertions, one would *be giving a definition* of the sentences which form the first part of these assertions:

(1) The sentence 'The sun is larger than the moon' means neither more nor less than that the sun is neither smaller than the moon nor as large as the moon.

(2) The sentence 'The sun is not smaller than the moon' means neither more nor less than that the sun is either larger than the moon or as large as the moon.

(3) The sentence 'A is a sibling of B' means neither more nor less than that A is either a brother or a sister of B.

(4) The sentence 'A is a brother of B' means neither more nor less than that A is a sibling of B but not a sister of B.

(5) The sentence 'A is a brother of B' means neither more nor less than that A is a male sibling of B.

I should say that, according to traditional logic, the three pairs of assertions, (1) and (2), (3) and (4), (3) and (5), yield circular definitions, and Prof. Moore would not recognise them as definitions at all.[1]

I found it hard, from the beginning, to believe that the greater total number of conceptions mentioned in the sentence used to express *p* should be a sufficient condition for regarding this sentence as a definition of *s*, disregarding the (relative) complexity of these conceptions. And the contradictions to which Prof. Moore's statement apparently leads but strengthened my doubts.

(3) Prof. Moore's technique for using quotation-marks is somewhat strange and leads to confusions. The distinction between use and mention of signs should by now be commonplace, and the dangers of disregarding it have been sufficiently clarified and exemplified by Frege, Carnap, Tarski, Quine and others.

[1] *Cf.* p. 197. I could, of course, have chosen much simpler examples such as defining 'large' by 'not small' and 'small' by 'not large', but I preferred to stick to Prof. Moore's examples.

Here are some more examples of the confusions resulting from such disregard: After having decided (p. 195) to call the sentence "At least one person is a King of France, at most one person is a King of France, and there is not anybody who is a King of France and is not wise"—"U", Prof. Moore says (p. 198): "Instead of writing U preceded by "that" and *not* putting inverted commas round it, I might have written, instead of "that", the words "the proposition", and followed these words by U *in inverted commas*". Whereas the expression 'writing U without inverted commas' is, though unfortunate, perhaps no more objectionable than 'writing 'burst' without 'r'', the expression 'writing U in inverted commas' is really confusing, as Prof. Moore's intention apparently was simply 'writing U' (because, according to him, U has already inverted commas!). Connected with this mistake are two other confusing usages. U, "in inverted commas", can be *either a proposition or a sentence* (which are different concepts, as used by Prof. Moore, a sentence *expressing* a proposition), according to the last cited sentence, and to distinguish between these possibilities we must, apparently, always prefix the word 'proposition' or the word 'sentence', which is, to say the least, very cumbersome. And, according to Prof. Moore, U can be used sometimes "*merely* as a name of itself", whereas it is not so used when, "in inverted commas", it is preceded by the words, 'the proposition'. Now, a sign might be used *autonymously*,[1] though it is perhaps preferable to avoid, whenever possible, such usages. But, as a matter of fact, Prof. Moore did *never* intend to use U "*merely* as a sign of itself", because even when U, "in inverted commas", is preceded by the words 'the sentence' or 'the words', it is used as a name of the *proposition* U, which is, unfortunately, also written "in inverted commas". And I do not believe that it is "correct" English usage to say, in such a case, that U is used "*merely* as a sign of *itself*".[2]

(4) Another objectionable formulation of Moore's is contained in his statements (p. 199) "We are never giving a definition, if

[1] *Cf.* R. Carnap *The Logical Syntax of Language*, 1937, p. 156f.

[2] For obvious reasons, I had to adapt my own usage of quotation marks in this article, partly to Prof. Moore's standards. Therefore, most of the sentences of this paragraph and of the following one, would be either false or (syntactically) nonsensical, according to the standards of the above-mentioned logicians. But I hope that the careful and interested reader will be able to make the necessary transformations for himself. For the same reasons, my use of 'sentence' and 'proposition' will not be consistent, and in them lies the cause of my annoyingly frequent use of double inverted commas and italics.

we merely say of one expression that it means what is meant by another. For, if this is all we are saying, a hearer or reader can understand us perfectly without needing to understand *either* of the expressions in question" and (p. 200) "Since it is not necessary, in order to understand such a statement,[1] that either sentence be understood, it follows that such a statement is *never* a definition". Now, it seems to me perfectly clear, that even if we *merely* say of one expression that it means what is meant by another, and do not, therefore, by this statement, give a definition to those who do not understand *either* of the expressions in question, we might nevertheless give by it a definition to those who understand *at least one* of the expressions in question. And though by such a statement [2] we may not give a definition to *every* hearer or reader, because it is not necessary, in order to understand the statement (in one sense of 'understand'), that either expression be understood (in another sense of this word), we might give by it a definition to those who should happen to understand at least one expression. In addition, I can hardly suppose that somebody who is given by Prof. Moore the definition "The expression 'is a triangle' *means* 'is a plane rectilinear figure, having three sides'" and understands this sentence *as a definition* (which entails that he understands the meaning of the expression 'is a plane . . .'), would not understand the statement "The expression 'is a triangle' *means what is meant* by the expression 'is a plane . . .'" *as a definition*. ('Being a definition' would accordingly be a *pragmatical* property of a statement, relative to the knowledge of the hearer or reader. But I think it is preferable to reserve this expression for the corresponding *syntactical* property together with 'being a possible definition', in analogy to a proposal made by Tarski [3] in a somewhat different context, and to use for the pragmatical property the expression 'being understood as a definition'.)

(5) Prof. Moore, in trying to prove that the proposition W, namely "The sentence 'At least one person is a King of France' means that at least one person is a King of France" is *not* a tautology, as one might be tempted to think, gives two reasons. His first reason is (p. 202): "W is the same proposition as "*Les mots* 'At least one person is a King of France' *veulent dire qu'une personne au moins est un roi de France*". But I think it is quite obvious that this proposition is not a tautology;

[1] Namely, the logical equivalence of two sentences written in a book in an unknown language. [2] *Cf.* the preceding note.

[3] *Einige methodologische Untersuchungen ueber die Definierbarkeit der Begriffe*, Erkenntnis 5 (1935), p. 81. Tarski's term is 'eventuelle Definition'.

and since it is the same proposition as W, it would follow that W is not either". Now, I think that Prof. Moore is right in denying the tautological character of the proposition W either in its purely English or in its mixed Anglo-French version, but, I do not believe he is right in assuming that, in the mixed version, the non-tautological character is more obvious than in the purely English one. or, to put it otherwise, I think that Prof. Moore's psychological attitude is not justified by the "correct" usage of language. The trouble is that Prof. Moore consistently fails to indicate the language, in reference to which he makes his statements, a procedure whose extreme importance has been sufficiently brought out by Carnap.[1] This failure is of no great importance in the greater part of Prof. Moore's article, as the reference-language is quite clearly understood to be the "correct" English everyday-language, but this conjecture is out of place, of course, in the case of the mixed version of W. I guess, therefore, that the reference-language in our case would be what we might call 'the "sum" of the "correct" English and French everyday-languages'. I think it is quite obvious that in reference to *this* language, the tautological or non-tautological character of the *mixed* version of W is *just as obvious* as the corresponding character of the *English* version in reference to the "correct" English everyday-language.

Prof. Moore's second reason is (p. 202): "I think it is also obvious on reflection, that the sentence Z ("At least one person is a King of France") *might*, quite easily, *not* have meant that at least one person is a King of France. To say that it does mean this, is to say something about the correct English use of the words which occur in Z and of the syntax of Z. But it might easily not have been the case that those words and that syntax ever were used in that way: that they are so used is merely an empirical fact, which might not have been the case." Now, this argumentation seems to me so fantastically absurd and confused, that I begin to doubt whether what appeared to me quite clearly understood up to this moment, namely, that Prof. Moore's normal reference-language is the "correct" or "established" English everyday-language, is after all the case. To put it in the form of a dilemma. Either (*a*) the reference-language is "correct" English. Now this contains the *semantical* rule: "If *s* is any sentence which expresses a proposition *p*, then *s* means *p*". In that case W is *semantically valid* though not *tautologous*; for the latter term designates, according to Carnap's and Quine's proposal and perhaps even according to "established" usage,

[1] *Cf. e.g. Syntax*, p. 299.

only a certain sub-class of *syntactically* valid propositions. (This is *my* reason for denying the tautological character of W, but not Prof. Moore's !) It is true indeed, that the proposition "W is semantically valid" can be validated only empirically. For the *sentence-event*[1] W (which is printed on pp. 201-202 of Prof. Moore's article) is a certain physical object, and so its accordance with the above-mentioned semantical rule can be established only empirically. Similarly, in reference to the same language, the proposition "A dog is a dog", which we will call 'D', is syntactically valid, and even tautological according to "established" usage. For "correct" English contains the syntactical rule "If *a* is any entity, then *a* is *a*". Yet the proposition "D is syntactically valid" can be validated only empirically. since the sentence-event D (which is printed as part of this sentence) is a physical object and its accordance with the above-mentioned syntactical rule can be established only empirically Or (*b*) the reference-language is just ordinary English everyday-language, in which case I can not see how *any* rule could be established and therefore how *any* sentence could be tautologous. I believe it is plain that applying any terms like 'tautologous', 'self-contradictory', etc., involves the use of certain explicitly stated or implicitly-assumed rules.

(6) Two pages further (p. 204), Prof. Moore argues that to make a certain assertion is "absurd for the same reason for which it is absurd to say such a thing as "I believe he has gone out, but he has not" is absurd.[2] This, though absurd, is not self-contradictory; for it may quite well be true. But it is absurd, because, by saying "he has not gone out" we *imply* that we do *not* believe that he has gone out, though we neither assert this, nor does it follow from anything we do assert. That we *imply* it means only, I think, something which results from the fact that people, in general, do not make a positive assertion, unless they do not believe that the opposite is true: people, in general, would not assert positively "he has not gone out", if they believed that he had gone out. And it results from this general truth, that a hearer who hears me say "he has not gone out", will, in general, assume that I don't believe that he has gone out, although I have neither asserted that I don't nor does it follow, from what I have asserted, that I don't. Since people will, in general assume this, I may be said to *imply* it by saying "he has not gone out", since the effect of my saying so will, in general,

[1] *Cf.* R. Carnap *Introduction to Semantics*, 1942, p. 6.

[2] The last two words of this sentence, namely 'is absurd', must of course be cancelled, to make sense.

be to make people believe it, and since I know quite well that my saying it will have this effect".

The sense of 'imply', which is discussed in the quoted passage (let us call this sense '*pragmatical*') "far from being mysterious"[1] —here I whole-heartedly share Prof. Moore's opinion—seems to me to be of the utmost importance in any linguistic behaviour-situation, so I intend to clarify it myself, as Prof. Moore's explanations seem to me to be unsatisfactory and confused, in the quoted passage as well as in the other place known to me.[2]

Let us assume that the following two *pragmatical* laws are highly confirmed:

(*a*) If, during a discussion, one of the participants starts to shout, he is excited.

(*b*) If, during a discussion, one of the participants starts to use abusive language, he is excited.

Now, let us imagine a marital dispute, during which Mary says to her husband "Henry, please, don't get excited", to which Henry responds by shouting "I am not excited" or by saying quietly "I am not excited, damn you". In both cases, Mary, we all, and even Henry himself (after having calmed down), will be fully justified in inferring that he *is* excited.

I am afraid that for exact evaluation of the situation here involved, no less rigorous terminology than, say, that of Carnap's "Syntax" will do. So let us start:

In the following three definitions only, 'implies'[3] will be used as expressing approximately that relation between sentences which, as a relation between either sentences or propositions, is called 'entails' by Moore, 'strictly implies' by Lewis, 'L-implies' by Carnap, 'logically implies' by other logicians.

(1) We shall say that $\mathfrak{S}_1$ implies $\mathfrak{S}_2$ in relation to a sentence-class α_1 (shortly, $\mathfrak{S}_1$ α_1-*implies* $\mathfrak{S}_2$), if, and only if, $\alpha_1 + \{\mathfrak{S}_1\}$ implies $\mathfrak{S}_2$.

(2) We shall say that $\mathfrak{S}_1$ *induces* $\mathfrak{S}_2$ if, and only if, there exists a highly confirmed α_1 such that $\mathfrak{S}_1$ α_1-implies $\mathfrak{S}_2$.

(3) We shall say that $\mathfrak{S}_1$ *Pr-induces* $\mathfrak{S}_2$ if, and only if, there exists a highly confirmed α_2 of pragmatical laws (where a law is called 'pragmatical', when it refers to users of language, roughly stated), such that $\mathfrak{S}_1$ α_2-implies $\mathfrak{S}_2$.[4]

[1] *Cf.* Prof. Moore's "A Reply to my Critics", *The Philosophy of G. E. Moore*, The Library of Living Philosophers, vol. iv (1942), p. 542.

[2] *Op cit.* pp. 542-543.

[3] In accordance with Quine's usage in *Mathematical Logic*, 1940, § 5.

[4] I hope to publish shortly a more detailed and exact study of the concepts here involved.

Let $\mathfrak{S}_3$ be 'Henry is excited', $\mathfrak{S}_4$ be 'Henry shouts $\sim\mathfrak{S}_3$',[1] $\mathfrak{S}_5$ be 'Henry says $\sim\gamma_3$ ⌢ 'damn you'' (where '⌢' designates *concatenation*[2] of expressions), α_2 be the class of the two above-mentioned pragmatical laws. Then, according to (3), we get: $\mathfrak{S}_4$ Pr-induces $\mathfrak{S}_3$ and $\mathfrak{S}_5$ Pr-induces $\mathfrak{S}_3$.

Since I believe that 'Pr-induces', as here defined, explicates fairly well that sense of 'implies' which is intended by Prof. Moore in this connexion, I shall return to use 'implies' and 'entails' in Moore's sense. It is important to notice that 'implies', though referring to a *pragmatical* class of sentences, is still a *strictly logical* relation, just as is 'entails', upon which it is based.

The interesting situation that Henry's shouting $\sim\mathfrak{S}_3$ *implies* $\mathfrak{S}_3$ may be specified by judging Henry's behaviour as *absurd*, since his *shouting* a certain expression implies the *contradictory* of that sentence. Similarly, by saying $\sim\mathfrak{S}_3$ ⌢ 'damn you', Henry *implies* $\mathfrak{S}_3$, and we may again specify this related situation by judging Henry's behaviour as *absurd*, since his saying a certain sentence implies the contradictory of (part of) it. (By the way, we should not have called his behaviour 'absurd', if by saying (not *asserting*) $\sim\mathfrak{S}_3$ with a certain twinkling of the eyes he had been supposed to imply that the proposition he intended to *assert* was the contradictory of that which he *said*.) These cases are remarkably different, since, in the first case, the implication is based wholly uopn the *mode of saying* and not at all upon what is said, so that we should have implied that Henry is excited even of he had *shouted* "I am going now to the pub", whereas our implication in the second case is based upon *what* he said (though not upon that part which is contradicted by implication). In both cases, assuming that the imagined disputes have actually taken place, we should also infer that since $\mathfrak{S}_4$ and $\mathfrak{S}_5$ are true, therefore $\sim\mathfrak{S}_3$ is false.

We are now ready to return to the example given by Prof. Moore in the quoted passage. Let 'P_6' and $\mathfrak{S}_8$ be 'Bill has gone out', $\mathfrak{S}_7$ be 'Dick believes $\mathfrak{S}_6$, but $\sim P_6$',[3] $\mathfrak{S}_8$ be 'Dick says $\mathfrak{S}_7$',

[1] I have replaced 'I am not excited' by 'Henry is not excited' to avoid the pitfalls connected with the use of "egocentric" (Russell) or "indicator" (Dr. Nelson Goodman) words, like 'I', 'this', etc. The '$\sim$' in '$\sim\mathfrak{S}_3$' is *autonymous*. To avoid this usage I could have made use, *e.g.*, of Quine's *corner*-notation.

[2] For this use of the *arch*, *cf.* Quine *Mathematical Logic*, § 53.

[3] Of course *not*: 'Dick believes $\mathfrak{S}_6$, but $\sim\mathfrak{S}_6$', which would be complete nonsense. I make use of Carnap's proposal (*Syntax*, p. 248) to interpret 'Dick believes P_6' as 'Dick believes $\mathfrak{S}_6$'. The argument is not altered thereby.

$\mathfrak{S}_9$ be 'If X is human and X says $\mathfrak{S}$, then X does not believe $\sim\mathfrak{S}$'. On Prof. Moore's assumption that $\mathfrak{S}_9$ is a (generally) highly confirmed pragmatical law, we get: $\mathfrak{S}_8$ implies 'Dick does not believe $\mathfrak{S}_7$'. Let 'P_7' be the designatum of $\mathfrak{S}_7$ then, according to Moore, we may re-formulate the last sentence as: $\mathfrak{S}_8$ implies 'Dick does not believe that P_7'. Since 'P_7' is a conjunction of two propositions ('but' may, in this case, be safely replaced by 'and'), we get: $\mathfrak{S}_8$ implies 'Dick does not believe that Dick believes $\mathfrak{S}_6$ or Dick does not believe $\mathfrak{S}_6$', which may be simplified to: $\mathfrak{S}_8$ implies 'Dick does not believe $\mathfrak{S}_6$'. Our final result is therefore: 'Dick says 'Dick believes $\mathfrak{S}_6$, but $\sim P_6$'' implies 'Dick does not believe $\mathfrak{S}_6$'. We are therefore entitled to judge Dick's behaviour as *absurd*, since his asserting a certain sentence implies the *contradictory* of (part of) it. And we may infer that since $\mathfrak{S}_8$ is true, 'Dick does not believe $\mathfrak{S}_6$' is true, i.e. (according to the semantical truth definition) Dick does not believe $\mathfrak{S}_6$.

I admit that the preceding section was rather complicated, but I do not believe that Prof. Moore's confused and confusing argument could have been explicated more simply. As one example of the inherent confusion let us investigate the second sentence of the quoted passage: "This, though absurd, is not self-contradictory; for it may quite well be true". 'This' seems to be short for 'to say such a thing as "I believe he has gone out, but he has not"', since 'absurd' is used by Prof. Moore (correctly, I believe) to characterise a linguistic behaviour rather than a sentence; 'it', on the other hand, must refer to the sentence "I believe he has gone out, but he has not", since 'true' characterises sentences (or propositions) but not behaviour. Though this interpretation now seems to me evident, it took me rather a long time to assure myself of the necessity to assume that Prof. Moore has been grammatically careless. But what about 'self-contradictory'? This term should refer to the behaviour; otherwise the grammatical (or perhaps logical) blunder involved would exceed by far what may be excused by carelessness. But, on the other hand, Prof. Moore has never used this term, at least in this article, for that purpose, and several times, (e.g. p. 203 and p. 205) he has used it for characterising a proposition. I am really at a loss, but I prefer to interpret Prof. Moore's intention in the second sense, though I believe that, generally, 'self-contradictory' may be used in the first sense too. I am led, therefore, to assume that what Prof. Moore intended to say was: "To say such a thing as "I believe he has gone out, but he has not" is absurd, though the proposition "I believe he has gone out, but he has not" is not self-contra-

dictory, as it might quite well be true". This proposition is true—if by "it may quite well be true" is meant no more than that it is a *synthetical* (factual) proposition, and therefore not necessarily false, though, as things are, it is false. And it is not only true but even important. For it shows that a certain linguistic behaviour, viz. asserting a proposition, may be *absurd* not only when the asserted proposition is self-contradictory or absurd (in which case the epithet 'absurd' is transferred from the 'proposition to the act of asserting it), but also when it only *implies* its contradictory. ('An absurd proposition' is synonymous with 'a self-contradictory proposition', as is 'absurdum' in "reductio ad absurdum".)

After this digression, let us return to Prof. Moore's argument in the quoted passage, in its restated version. Is it sound? I am not sure, for two reasons: (*a*) I doubt whether $\mathfrak{S}_9$ is highly confirmed, though I should not doubt that $\mathfrak{S}_{10}$, namely, 'if X is human and X says $\mathfrak{S}$, then X believes $\mathfrak{S}$', is (generally) highly confirmed. $\mathfrak{S}_9$ does not follow from $\mathfrak{S}_{10}$, nor is $\mathfrak{S}_9$ even Pr-induced by $\mathfrak{S}_{10}$. 'A believes $\mathfrak{S}$' does not imply (in Quine's sense) 'A does not believe $\sim \mathfrak{S}$', and I do not even think that 'A believes $\mathfrak{S}$' Pr-induces 'A does not believe $\sim \mathfrak{S}$', since I am not sure that a certain positive psychological attitude always involves (empirically) a certain negative attitude. (*b*) I am not sure that we are entitled to simplify 'Dick does not believe that Dick believes $\mathfrak{S}_6$ or Dick does not believe $\mathfrak{S}_6$' into 'Dick does not believe $\mathfrak{S}_6$'.

I was myself astonished to see what complicated structures resulted from my restatement of Prof. Moore's argument but one never can tell what confused and inexact formulations may lead to, when submitted to a certain tentative clarification. Anyhow, I do not think it is worth while to investigate further into the psychological soundness of the argument, since Prof. Moore's intention in using it was merely to show that we may sometimes, by asserting a certain proposition, *imply* its contradictory, and this fact has been sufficiently illustrated by my examples in the beginning of this paragraph.

As a conclusion of this criticism, I should like to summarise its positive results:

(1) There is an important *pragmatical* sense of 'implies', namely what we called 'Pr-induces', which is different from the other important senses of this term. This hitherto rather neglected sense should prove of decisive value in discussing, not only ethical problems,[1] but any problem involving linguistic

[1] *Cf.* Moore's *Reply*, pp. 540-543.

behaviour, or even—after a certain extension of its applicability—sign behaviour generally. It is possible and even probable that, by asserting certain sentences, which are 'meaningless' according to some empiricist criterion of 'meaning' (e.g., sentences of a metaphysical or aesthetic nature), one may nevertheless *imply* sentences which are perfectly meaningful, according to the same criterion, and are perhaps even true and highly important.[1]

(2) There is an important *pragmatical* sense of 'absurd', in which it refers to a certain linguistic behaviour, where asserting a proposition *implies* its contradictory.

(7) I asked several language-teachers to give me their opinion about Prof. Moore's statement (p. 211) that to translate the French sentence "*Le soleil est plus grand que la lune*" into the English sentence "The moon is smaller than the sun" (to be called 'E') would be definitely incorrect. None of them assented, but most of them were ready to consent to my proposal, that though such a translation would by no means be *definitely* incorrect (I assume that 'definitely', as used by Prof. Moore in this context, means 'in *every* sense of this word'), and on the contrary quite correct, according to the semantical rules of the "sum" of the "correct" English and French everyday-languages, it would be *inferior* to the *literal* translation into "The sun is larger than the moon" (to be called 'E_1'), since a literal translation is, *ceteris paribus*, preferable to a non-literal one. I, personally, should hesitate to call such a translation even 'pragmatically incorrect'.

(8) I find it extremely difficult to imagine the reasons which led Prof. Moore to state (p. 211) (1) that "it would be definitely incorrect to say "The proposition '*Le soleil est plus grand que la lune*' both entails and is entailed by the proposition 'The sun is larger than the moon'", or to say that "we have here *two* propositions which are logically equivalent"; (2) at the same time to doubt (pp. 210-211) "whether it is incorrect to say that the proposition 'The sun is larger than the moon' is *one* proposition, and the proposition "the moon is smaller than the sun" is *another* proposition"; and finally, (3) to assert "And yet I do not think we can say that the sentence "The sun is larger than the moon"

[1] To provide just one example of an application of this thesis, *cf.* the recent discussion between Mr. Morris Weitz and Mr. Raymond Hoekstra in *Philosophy and Phenomenological Research*, vol. v, 3, March 1945, about an article by the former "Does Art tell the Truth?", published in vol. iii, 3, March, 1943, of the same Journal. How about "Artists (normally) do not *assert* true sentences, but sometimes, by uttering false or even "meaningless" sentences, *imply* true sentences"?

brings before the mind any ideas which are not brought before it by the sentence "The moon is smaller than the sun", nor yet that the latter brings before the mind any ideas which are not brought before it by the former". I interpret the last assertion to mean that the propositions E and E_1 are, in addition to being semantically equivalent, also pragmatically equivalent. I can therefore by no means see what should prevent the sentences E and E_1 from expressing the *same* proposition, in *every* "correct" usage of the word 'same'.

I now proceed to my final remark. I believe I am entitled to infer from Prof. Moore's assertion (p. 212) "I must, therefore, confess that I am unable to point out where the fallacy lies in these arguments . . ." that he is quite aware of the fact that *his* system of rules governing the "correct" use of English is *inconsistent*. This point is rather trivial, since the inconsistency of "correct" everyday-language has been elaborated often enough—Russell's "Theory of Description", e.g. was partly intended to correct some of the inconsistencies of "correct" English and may "be plausibly interpreted as a contribution to the reform of common syntax",[1] but I do not think that either Prof. Moore or most of the other adherents of Contemporary British Analytic Philosophy are ready to draw the consequences from this fact. *The reform of common syntax* (and of common semantics and pragmatics) *cannot be accomplished by direct logical analysis alone*. This task should be approached by construction of consistent language-systems, by elaborating a "pure semiotic", which will supply the necessary terminology, etc.: shortly, by the methods of *logical empiricism*. Now, I do not deny the value of analysing (in a much more general sense of this word than Moore's) and reforming everday-language; on the contrary, these tasks are of the utmost importance, since everyday-language—"Universal-jargon", to use Neurath's term—is ultimately the only medium for constructing, interpreting, and comparing artificial language-systems, and therefore (and for many other reasons) absolutely indispensable. But Prof. Moore's method of approaching these tasks is just as Sisyphian as a physicist's who should start to "analyse" the facts of free fall, without constructing or imagining artificial ("ideal") conditions (vacuum, etc.), in the manner of Galilei.

"The direct analysis of these (incredibly complicated word-languages), which has been prevalent hitherto, must inevitably fail, just as a physicist would be frustrated were he from the

[1] *Cf.* Max Black "Russell's Philosophy of Language", *The Philosophy of Bertrand Russell*, The Library of Living Philosophers, vol. v (1944), p. 242.

outset to attempt to relate his laws to natural things—trees, stones, and so on. In the first place, the physicist relates his laws to the simplest of constructed forms; to a thin straight lever, to a simple pendulum, to punctiform masses, etc. Then, with the help of the laws related to these constructed forms, he is later in a position to analyse into suitable elements the complicated behaviour of real bodies, and thus to control them. One more comparison: the complicated configurations of mountain chains, rivers, frontiers, and the like are most easily represented and investigated by the help of geographical co-ordinates—or, in other words, by constructed lines not given in nature. In the same way, the syntactical property of a particular word-language, such as English, or of particular classes of word-languages, or of a particular sub-language of a word-language, is best represented and investigated by comparison with a constructed language which serves as a system of reference."[1]

It is as an illustration of this thesis that my article was written.

I hope I have "implied" that the whole idea of the existence of a unique "correct" English everyday-language is illusory, and that even if, by some miracle, one of the various sets of "correct" linguistic rules should be recognised as *the* "correct" one, there would still be no good reasons to assume this set to be consistent, and very good ones to assume it to be inconsistent.

I further hope I have shown that many of Prof. Moore's mistakes could have been avoided and many of his puzzles easily solved by the recognition of the multi-dimensional character of language and by the use of the most elementary tools of "pure semiotic", e.g. by the use of 'syntactical', 'semantical', 'pragmatical' in expressions like 'syntactically valid', 'semantically equivalent', 'pragmatical law'.

The direct analytical approach seems to me a deplorable waste of time and energy, and if I should succeed in persuading some of its capable adherents to try more promising approaches, writing this article will have been no waste of energy on my part.

[1] R. Carnap *Syntax*, p. 8.

V.—DISCUSSION.

ACHILLES AND THE TORTOISE.

A PARADOX is a philosophical mystery story, and in its apparently unsolvable contradiction lies much of its universal charm. Two statements each obviously true and yet each contradicting the other offer a challenge to the understanding that anyone with a vestige of intellectual curiosity cannot fail to accept. The persistent vitality of Zeno's paradoxes is evidence not only of this charm of mystery, but of the inherent difficulty in providing a really satisfactory solution. Andrew Ushenko, writing in MIND for April 1946, points to three discussions of them in this quarterly during the preceding five years and to a number of other proposed solutions. These perennial discussions indicate to him that a really satisfactory solution has yet to be obtained.

Zeno's paradoxes were part of his argument in defence of the philosophy of Parmenides, and—broadly speaking—were aimed not at directly supporting Parmenides' thesis, but at showing that the arguments of his opponents were open to as many if not more contradictions. Because of their knowledge of, and interest in, these broader philosophical aspects, many commentators on the paradoxes have wandered into philosophical by-paths instead of confining themselves to the simple arguments of the paradoxes themselves. Dr. Ushenko is himself not entirely guiltless in this respect in spite of quoting with apparent approval J. O. Wisdom to the effect that: "The problem is not to prove that Achilles catches the tortoise, because we know that he does, but to find out where the 'proof' that he does not is faulty and why it is convincing."

Typical of Zeno's paradoxes, and perhaps the widest known, is this of Achilles and the Tortoise to which Wisdom was referring. If a tortoise is one mile ahead of Achilles at some instant, and travels at a speed of one half-mile an hour while A travels at twice that speed, then A, in spite of his advantage in speed, will never overtake T. The proof offered by Zeno is that when A has reached the point originally occupied by T, T will be one half-mile ahead. When A reaches this latter point, T will be one quarter-mile ahead, and so on with decreasing distances forever. A never overtakes T because when he reaches the point where T was, T is always some distance—although perhaps a very short one—ahead.

These paradoxes are commonly referred to as arguments against the reality of motion, and it is this characterisation of them that leads many of their commentators into discussion beside the point. In the paradox of A and T, however, the existence of motion does

not seem to be questioned directly. Zeno admits it not only by assigning velocities to A and T in the statement of the problem, but concedes it as well in his subsequent argument, since he allows A to travel at constant velocity for at least the first mile. It seems justifiable, therefore, to seek the objective clearly put by Wisdom without digressing to discuss the possibility of motion. Zeno's objective in the paradox was apparently to typify the contradictions encountered in accepting motion rather than to directly disprove it.

A paradox always arises because of some concealed error, and the fallacy of which Zeno is guilty in his A and T paradox is the *ignoratio elenchi*, or irrelevant conclusion. He states he is going to prove that A never overtakes T, but what he really proves is something quite different. It is that the sum of the terms of a geometric progression, when the ratio is less than one, never quite reaches a definite limit although approaching it continually.

This new problem that Zeno slips surreptitiously into the place of the original is essentially a static one, and does not involve velocity at all. Quite possibly it was a deliberate sophism on Zeno's part. Although he talks of velocity in presenting his proof, it is really superfluous, and is merely the device he employs to introduce the fractional ratio for his geometric progression. In its simpler and more correct form, what Zeno proves is that if A and T are one mile apart at the start, and are moved in successive steps in such a way that A is always brought to the position T occupied at the beginning of the move, while T is moved one-half that distance ahead, A will never overtake T. This is undoubtedly true, and would probably be recognised as such by anyone even without knowing that the successive moves of both A and T form geometric progressions with a ratio of one-half.

That this argument is sound and the conclusion correct may be illustrated graphically. Zeno considers the relative positions of A and T over an infinite series of arbitrarily selected moments. If these moments of consideration are plotted as equally spaced intervals along the abscissa of a set of rectangular co-ordinates, and distances from A's starting point are plotted as ordinates, the argument becomes as shown in Fig. 1. As the number of moments considered increases, both A and T approach closer to a distance of two miles but never reach it, and at all positions T is closer to the two-mile point than is A. The successive positions of each—measured from the limiting point two miles ahead of A's starting point—form a geometric progression with a ratio of one-half, but A's series starts with 2 and T's with 1, and thus for corresponding terms T is always nearer the limiting point by one step of the progression.

In this argument velocity does not need to enter. Zeno makes it appear to be present by the tacit use of the relationship $s = vt$. Since the velocities of both A and T are assumed constant, the distances travelled in equal intervals are proportional to them, and this enables Zeno to speak of velocity when he really is using

only a succession of distances. In other words, he uses the relative velocities of A and T to derive the relative distances he moves them in the successive steps of his demonstration. Since the process he describes correctly leads to the conclusion he reaches, and since the listener has been misled into assuming that the velocities have actually been used in the demonstration, the paradox is established.

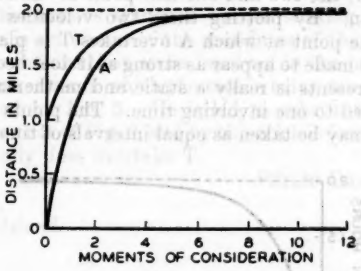

FIG. 1.

Velocity is the ratio of distance to time, and to depict a problem in velocities, distance is plotted along one axis and time along the other. For the problem as originally stated, the graph would be as shown in Fig. 2. Like Fig. 1, this has distance plotted as

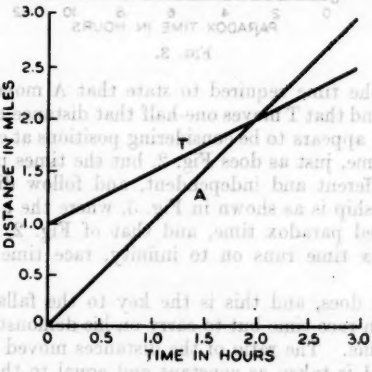

FIG. 2.

ordinates, but the abscissa scale is composed of equal intervals of time instead of arbitrarily selected moments as in Fig. 1.

Zeno words his argument in such a way that the position of T at any instant is always affected by that of A, and vice versa, while in a true velocity problem they should be entirely independent. In Fig. 2, for example, A reaches the one-mile point at the end of the first hour by Zeno's own admission, and by the same laws and

forces—and completely irrespective of what T is doing or where he is—A will reach the two-mile point at the end of the second hour, and the three-mile point at the end of the third hour, and so on. In an exactly similar manner, T has reached the one and one-half point—reckoning from A's starting position—at the end of the first hour, and similarly will reach the two-mile point at the end of the second hour, the two and one-half point at the end of the third hour, and so on. By plotting these two velocities independently as in Fig. 2, the point at which A overtakes T is plainly indicated.

Zeno's case is made to appear as strong as it does because, although the series he presents is really a static and mathematical one, it is readily converted to one involving time. The points of the abscissa scale of Fig. 1 may be taken as equal intervals of time, each interval

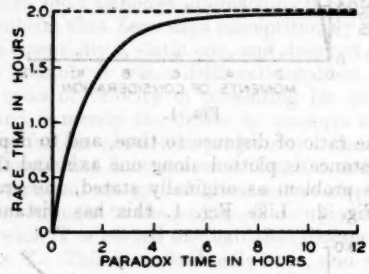

FIG. 3.

representing the time required to state that A moves to the point where T was and that T moves one-half that distance ahead. By doing this, Zeno appears to be considering positions at equal successive intervals of time, just as does Fig. 2, but the times used for Figs. 1 and 2 are different and independent, and follow diverging paths. Their relationship is as shown in Fig. 3, where the abscissa scale of Fig. 1 is called paradox time, and that of Fig. 2, the race time. While paradox time runs on to infinity, race time never reaches two hours.

What Zeno does, and this is the key to the fallacy, is to state the problem in race time but to carry on his demonstration in terms of paradox time. The ratio of the distances moved for each of the steps of Fig. 1 is taken as constant and equal to that between the two velocities of the original statement of the problem, and it is this equality that misleads the listener into the conclusion that Zeno has really produced a paradox. By the shift of abscissa scale made by Zeno, the time during which the race is being carried on is arbitrarily slowed down to permit Zeno to wander off in an infinite sequence of positions. He blocks race time at something less than two hours while carrying his listener along in paradox time.

Irrelevant conclusions derive their power to deceive because the

error is so worked into the wording of the argument as to raise a psychological barrier to detection. The listener becomes absorbed in the proof and perceives the argument to be sound. He neglects to notice that what is proved is not exactly what was supposed to have been proved. In this particular paradox, the listener sees plainly that, following the progression outlined, A will never overtake T. Zeno states his discussion in terms of velocities in the proper ratio, and the listener, as intrigued by the striking nature of the conclusion as he is convinced of the soundness of the proof, fails to observe that the infinite time series involved in the proof is not the same infinite time series in which the race is being run, but is related to it as indicated by Fig. 3. He is led to substitute the abscissa scale of Fig. 1 for that of Fig. 2, and by this trick is forced to conclude that A never really does overtake T.

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VI.—CRITICAL NOTICES.

Invisible Anatomy: A Study of Nerves, Hysteria and Sex. By E. GRAHAM HOWE, M.B., Fellow of the British Psychological Association. London: Faber & Faber Ltd., 1944. Pp. 233. 10s. 6d.

Psycho-analysis. By EDWARD GLOVER, M.D., Director, London Clinic of Psycho-analysis; Director of Research, The Institute of Psycho-analysis. London: John Bale Ltd., 1939. Pp. 139. 15s.

The Lady of the Hare: A Study in the Healing Power of Dreams. By JOHN LAYARD, M.A. Cantab. and Oxon., D.Sc. Oxon. London: Faber & Faber Ltd., 1944. Pp. 277. 12s. 6d.

Survey of Objective Studies of Psychoanalytic Concepts: A Report Prepared for the Committee on Social Adjustment. By ROBERT R. SEARS, Professor of Child Psychology, Child Welfare Research Station, State University of Iowa. New York: Social Science Research Council, 1943. Pp. 156.

1. PART I of *Invisible Anatomy* discusses, the author says, "the simplified patterns of the nervous system with special reference to that most widespread disorder which we know as 'nerves'". Part II "includes ten doctored case-histories, to illustrate the sort of experience that we suffer in our nervous and emotional disorders". Part III gives "my own idea as to the metaphysical background out of which all problems emerge, as variants of what I regard as our 'normality'". Part IV is "a more detailed study of the nature, cause, prevention and cure of that common emotional disease known as Hysteria". Part V "suggests some clues as to the meaning and mystery of sex, and how our lives can be conducted so as to avoid some of the more common pitfalls of ill health, unbalance and unhappiness".

"*Invisible Anatomy: A Study of Nerves, Hysteria and Sex.*" How well the title and the sub-title of this book hint at what is within! 'Anatomy'—science we trust, 'invisible'—mystery we hope, 'nerves and sex'—familiar words for what we know and wish someone would speak of. We are reminded of D. H. Lawrence's physiological mythology for psychological description. But this book has not the consistency of Lawrence. In it triangles and the central and autonomic nervous systems, snakes, chalices, some of the concepts of Freud and Jung, and stories of men, women and children

contented and distressed, are inextricably mixed in an attempt to describe, to order, the psychological ills of humanity. Is it more than the hocus pocus of a medicine man using scientific terms with the fearful facility of the inmates of mental hospitals? And yet, how simply and vividly Dr. Graham Howe tells the stories which illustrate his 'theories'. Certainly he knows the creatures he writes of and I feel that what he writes can give us more wisdom in dealing with ourselves and others. And yet, what is it that I deplore about this book even while I admire its courageous, kindly flow? The answer is that there is in it too much wallowing in mystery. I love mystery and Heaven forbid that we should become nervously shy of admitting metaphors and models to our scientific descriptions—lines of force and planetary atoms, censors and super-egos, all are welcome, and we know the crudities of misplaced simplicity which result when we are too determined to work only one explanatory apparatus. But we may be economical without being parsimonious. And we must be. We have good science when every model is a hard-worked machine for connecting observations. *Invisible Anatomy* is too lush for science, and it doesn't pretend to be literature—perhaps persons less academic than myself will accept it as neither. Certainly many laymen will get more from it than from Dr. Glover's *Psycho-Analysis*.

2. *Psycho-analysis.* The latter is not written for laymen, but for practitioners. Even to them I fear it will not mean much unless they have already read other books on psycho-analysis. Nevertheless it may do what the author hopes, namely, give some idea of the existing scope and future possibilities of the science. It is an outline, and, as Dr. Glover says, the task of condensing the theory and practice of psycho-analysis within about 140 pages is by no means easy.

The book is in three sections. The first has chapters on the Embryology, the Dynamic Aspects, the Structure, and the Economics of Mind and on Phases of Mental Development, Dreams and Symptomatic Acts, and Symptom Formation.

In Section II there are chapters sketching types of psycho-neurosis, psychosis, psycho-sexual disorder and other social disorders, and a chapter on the psycho-analysis of children.

Section III is on practical applications of psycho-analytic theory. It contains notes for the physician on the examination of a patient with a view to diagnosis, prognosis and recommendation of treatment, on the duration and cost of treatment, and on the various sorts of treatment. The three pages on this last point, which is treated under the heading *The Nature of Psycho-analysis*, are interesting and one wishes that there were more of them.

I think that the three "approaches"—the structural, the dynamic and the economic, which Dr. Glover says are essential to the full understanding of every mental event, should have been more fully explained because of the part they play in his presentation of the facts.

In the first place, it is not easy to see at once how these metaphors are to be worked in the description of human behaviour. However, a little care reduces this difficulty.

One may (*a*) describe the structure of a machine, say a hydraulic system, and then (*b*) tell where the water is in it at a given time, and then (*c*) state the laws in accordance with which the water flows from part to part of the system. One may (*a*) describe the structure of a society, for example, the king and queen, the priests, the barons, the pedlars, the butchers and bakers and the serfs, and then (*b*) say where the wealth now is in a given society, or say where it is liable to be in any society of a given age, and then (*c*) state the laws in accordance with which it moves or does not move from class to class. One may describe the structure of a simpler society—the family—the father and the mother, the elder brothers and sisters, the children, the wicked uncle, the step-mother. Here again we may describe (*b*) where the power and influence at a given time lies and also (*c*) the laws of how from time to time it shifts.

It may then strike one that the structure and dynamics of societies, *and especially of the family*, are to an unsuspected degree mirrored in individual minds—mirrored profoundly but in appalling, ridiculous and sublime caricature—a Moloch of blood and flames and the Madonna of the Eucharist.

Sometimes a society acts as if all power lay in the hands of the most babyish and animal members, and sometimes as if all power lay in the hands of strict old men, and sometimes it acts more as a whole—mostly when there's a war on. Sometimes a man is not himself and acts as if a babyish man or cunning animal had gained control—that's the id—sometimes as if an exacting parent, a sarcastic schoolmaster, or an implacable deity possessed him—that's the super-ego. Sometimes a man is more himself and acts more as a whole, a new whole which is not a combination but a synthesis of the id and the super-ego. Some are constantly at the mercy of the id, some are slaves to the super-ego, in some first one and then the other gains an unhappy victory in a continual struggle, and in some conflict and control have vanished into co-operation as with a man hunting hounds on a horse that knows the work and likes it.

So far so good. But if we now try to divide the description of the structure of mind from its dynamics and its economics, we are at once in difficulties. It is comparatively easy to describe the structure of a machine without describing its functioning, for example, a water mill without the water or the way it flows. It is far harder to describe the classes of society without describing where the wealth lies and how it flows. And it is impossible, as Stout remarked, to describe a mind without reference to what it minds, what it wants and how it reacts; that is, to describe a mind *is* to describe where its energy lies and how it flows. Consequently, if one is to divide the description of the structure of mind from the description of where the water of life in it lies and the laws of its flow, one must

divide (*a*) the description of the things a mind *might* be interested in, or the things *minds* are in fact *from time to time* interested in, from (*b*) the description of what they are interested in *at a given age* or what *a particular mind* is or was interested in at a particular time and from (*c*) the description of the laws in accordance with which interest is re-distributed. One might expect that an attempt to separate these three descriptions so very closely connected would break down—interfere with exposition and result in repetition. So it does, I think. Nevertheless, Dr. Glover is to be congratulated upon getting so much that is valuable into so small a book.

3. *The Lady of the Hare* by John Layard is in three parts. The first is a very interesting account of how an unhappy situation involving a woman, her sister, her daughter and her husband was successfully treated on Jungian lines in twelve interviews. A dream about a hare played an important part in this, and the second part of the book (pp. 100-229) is concerned with the significance of the hare in the world's mythological systems. The third part (pp. 230-248) is called "More Dreams about Hares and Rabbits—showing the mythological motives on which they are based and their relation to everyday life".

I cannot even summarize here the story of the treatment. The mother, Mrs. Wright, told her dreams, and the analyst, Mr. Layard, gently and carefully offered interpretations in such a way as to encourage her to use her own powers of interpretation. In the section on the subsequent development of the daughter (p. 96) we read that Mrs. Wright reports that Margaret is "bright and happy and not the Margaret we used to know". Before the treatment of Mrs. Wright, Margaret would hardly speak to anyone, wrapped herself up in her reading (Edgar Wallace, for preference) and always went out of any room her aunt came into. The husband also disliked the presence of the aunt but agreed with the mother that she could not be turned out because she had nowhere to go to. The first indication of a big change in Margaret was when she started to have daylight visions which Mr. Layard says were continuations and extensions of Mrs. Wright's night-time dreams. "These visions" he says, "quickly developed into what can only be termed 'second sight' combined with knowledge of certain aspects of her mother's past history obtained through visionary and auditory contact with her mother's father, who had died when Margaret herself was only three years old. This visionary perception of her maternal grandfather soon became merged with the traditional figure of Bonnie Prince Charlie . . . and this concept in turn was replaced by a higher concept still, that of the Heavenly Father, under whose direct guidance she now believes herself to be" (p. 98).

What happened in the twelve interviews with Mrs. Wright? The first answer we want is the one a cine-camera and a dictaphone would provide. But even if we had such a complete account of what happened in the interviews we might still ask "What

happened ? " in the way we might ask this of a chemist who had done something astonishing, even though we had had a clear view of all he did and were confident he had no rabbits up his sleeve. The chemist would tell us a story of molecules and atoms hurrying, clinging, and separating and he would tell it to us with the air of an engineer who tells us what happens inside the machine which so cleverly prints and pushes towards us for 2d. a ticket to the Marble Arch. But really, the chemist is quite different from the engineer. He has never seen anything different from the sort of incident we now observe with him. It's only that he has seen more of them. Nevertheless, fantastic as is his story, it serves its purpose, for, like the engineer's, it connects the incident before us with thousands of others with which we should never have connected it but for his myth of the molecules.

Well, what happened in the twelve interviews ? Mr. Layard does not in this book do much towards answering this. He writes a section called " Brief Theoretical Discussion " (pp.. 86-96) but it is so very brief and it is not all explanation. However, he writes enough to make me at least very confident that Dr. Graham Howe would write a very like account and Dr. Glover a very different one. Two questions arise : (1) How far are the different accounts incompatible ? (2) Which is the better ? or which is the better for this purpose and which for that ? I can't deal with these questions because I don't know enough about Freud and Jung nor about Mrs. Wright to guess what the accounts would be. But even if the accounts were written out for me I should, I believe, still find difficulty in describing the differences and the likenesses between them. However, I venture the following remarks. Margaret's attitudes to her mother, her aunt, and her father, were connected with her feelings to her mother's father and, on the other hand, with her feelings to Bonnie Prince Charlie and to her Heavenly Father.[1] Neither Dr. Graham Howe nor Mr. Layard nor Jung nor Dr. Glover nor Freud would deny the importance of either of these connexions. But a Jungian would stress, I believe, the connexions with the Heavenly Father and the Freudians would stress the connexion with the maternal grandfather. (And what about her own, earthly father ? Is it to be supposed that he had nothing to do with the case ?)

Eckerman in his report of conversations of Goethe says (Jan. 10, 1830) " This afternoon Goethe afforded me great pleasure by reading the scene in which Faust visits the Mothers. . . . Although I had heard and felt the whole, yet so much remained an enigma to me that I asked Goethe for some explanation. But he, as usual, wrapped himself up in mystery, as he looked on me with wide open eyes and

[1] " Duncker (1938) reported a reversal of children's food preferences following the reading of an animal story in which the mouse hero indicated a decided preference for the food previously unpreferred by the children." *Survey of Objective Studies of Psycho-analytic Concepts*, p. 56.

repeated the words 'Die Mütter! "Mutter" klingt so wunderlich.' 'I can reveal to you no more', said he, 'except that in ancient. Greece mention was made of the mothers as divinities. . . .'" Eckerman goes on to offer what he calls his view of why the passage has so powerful an effect. He says . . . "Could we imagine that that huge sphere our earth had an empty space in its centre . . . this would be the abode of these unknown goddesses . . . beyond all place . . . beyond all time . . . all souls and forms of what has been, or will be, hover about like clouds in the vast space of their abode. So are the Mothers surrounded; and the magician must enter their dominion, if he would obtain power over the form of a being and call back former existences in seeming life."

This reminds me both of Jung and of Freud, of Dr. Graham Howe and of Dr. Glover. It manages to bring out at the same time both a man's feelings as a child, as an infant animal, to his earthly mother, and his feelings as a religious adult to the All-mother, who came out of chaos and gave birth to the Gods. And we need both these connexions if we are to understand a man's attitude to his mother or to the woman with whom he has just fallen in love. But though we need them both we must note that the one explains in a way for which the other only prepares us.

Suppose a man tells us of a horse that its back tendons are sprained. Then he not only tells us what symptoms we may expect, he also gives us in this description of the present an outline of the history which he guesses led to it. Suppose another man tells us of a horse that it is now in a peculiar condition since, though the Evil Eye has fallen upon it, gradually the Life Force is triumphing in it. This is descriptively useful if we know the sort of patterns of unfortunate symptoms of mysterious origin associated with the Evil Eye, and the sort of recovery processes, equally mysterious, which are attributed to the Life Force rather than to Harvey's Condition Powders. But this 'explanation' though it describes the horse's condition tells nothing of its cause although it pretends to do so.

Likewise—to say that there is in a man's attitude to the woman with whom he is in love something of his attitude to his mother and something of his attitude to his father or the cat he used to cuddle in his cot, is not merely to analyse his attitude, it also hints at its aetiology. On the other hand, to say of a man who has never heard of the Virgin Mother nor of the Venusberg that in his attitude to the woman with whom he is in love struggles his attitude to the Madonna and his attitude to the Venus of the Venusberg, is to describe his attitude but not to say whence it comes. Such a description analyses his attitude, places it with reference to psychological co-ordinates and so connects it with other attitudes and so prepares the way for a causal guess about it. But it does not itself make a causal guess. It does not even lead to one unless and until we venture not a description merely but a causal story for the power of the Venus and the Virgin.

Of course a hypothesis may be both descriptive or connective and also explanatory. In fact most hypotheses are thus mixed. We may know that a man has been brought up to love and reverence the Virgin and then by saying that *in* his attitude to the woman with whom he is in love is his attitude to the Virgin we are not merely describing his attitude we are also saying something of where it comes from, just as when we say that in his attitude to the woman with whom he is in love is his attitude to his mother. But the fact that in a hypothesis may be combined the functions of connecting description and of explanation, though it makes it less easy to apply the distinction between success at the one job and success at the other, does not make it less important—not if we are to arrive at a just estimate of what a given hypothesis accomplishes and what it does not and of how hypotheses conflict and of how they are to be supported and how refuted.

If we are told that a woman's eccentric act is due to her implacable super-ego then this hypothesis not only classifies her act but also outlines its aetiology. But if we are told that it comes from her neglect of her faculty of introverted intuition (compare Mr. Layard p. 235) this connects her act with others,[1] but does not outline an aetiology until we are provided with an account of the sort of past which produces such neglect. In short, we don't "resolve the present into the past" if we don't. Mr. Layard, quoting Mr. Johan Jacobi, says (p. 22) that Freud succeeds (or would he say 'tries to succeed?') in doing so.

He also says that Jung builds up "out of the actual situation towards the future". And that may well be so. For the fact that a psychologist does not provide a causal account of the disorders he treats by no means shows that "his therapeutic notions are . . . inept", as Mr. Philip Toynbee says Jung's are.[2] On the contrary, his notions of what to do for a patient may be just the thing. Perhaps he can't tell us *what happened* when the cure worked, perhaps he can't tell us what past events made effective the words of the interviews any more than he can tell us what past events made the symptoms appear. But the curative power of talk *in* a cure is not necessarily connected with the truth of that talk, still less with the adequacy and correctness of talk *about* the cure when the magician tries to say why it worked. Revivalist preachers have cured people quicker than any analyst, but this does not in itself establish the truth of the doctrines they taught.

[1] Of course, a hypothesis does this only if the meanings of terms involved in it, such as 'introverted intuition', have been taught, and that not by connecting them with other mysterious words (compare Mr. Layard's quotations from Jung on pp. 111, 112) but by connecting them with familiar words. When and in so far as the meanings aren't thus taught, either by synonyms or by samples, then we had better call mumbo-jumbo by its name and have done with it.

[2] "Polemic", May, 1946, pp. 54, 55.

One might indeed suspect that often a cure may be more quickly effected by connecting present reactions with other present reactions than by explaining them in connecting them with past reactions. It is not only that the past ones occurred so long ago that it is hard to remember what they were, it is also that the patient may be averse to remembering them and that his arrangements for avoiding doing so are more firmly established than his arrangements for keeping present reactions separate from each other. One might suspect also that cure by connecting contemporary patterns would give a less stable psychical equilibrium and one of a different quality and that it might in certain cases fail. Even so, speed is a serious consideration. Surely what treatment it is wise to try depends upon the case (see Glover, p. 126).

No one must suppose from all this that Jungian psychologists never refer to the past and no one must forget that Freudian psychologists consider most carefully contemporary connexions. What, for example, is insistence upon the transference connexion if it is not insistence upon a connexion as present as possible?

In Part II of his book Mr. Layard collects a vast amount of material about the mythology of the hare. I have not read it all. What I have read shows that though the hare stands for like things in different times and places, it also stands in different contexts for very different things. Consequently one cannot be confident about what it stands for in a given instance, for example, in a dream, without additional information about the context of the dream. It seems to me that in Part III, Mr. Layard interprets certain features of a number of dreams as if such interpretation were a much simpler process than it is and as if symbols were much more regular in their significance than they are. I had better confess that I get the feeling that in some degree he spins the interpretations out of his 'inner consciousness'. There is, I know, no harm in that if there's a check-up process—but is there?

4. Mr. Robert R. Sears aims—thank heaven—at having a check-up process for every question. *Survey of Objective Studies of Psychoanalytic Concepts* is "a summary and appraisal of published investigations, both experimental and observational, that relate to problems and concepts deriving from Freudian theory".

Chapter I is on oral, anal and genital eroticism. Figures are given for the frequency of thumb-sucking, nail-biting, and oral gestures in various groups; figures for the frequency of thumb-sucking among 122 children with various degrees of opportunity for sucking during the regular feeding process; and figures for—but I cannot summarize the data. Mr. Sears says in conclusion to this chapter (p. 21) "Freud first applied the properties of adult sexuality to infantile activities centred around the oral and anal-urethral body zones and then assumed that there was a specific quantum of pleasure-seeking that could be channelized through the various zones, making one a substitute for another. The evidence cited here supports the

general correctness of the first point, but throws less light on the latter."

Chapter II is on the erotic behaviour of children. Here, for example, is a table giving the rank order of frequencies with which different types of sex questions were asked by the 1797 children of 981 Minneapolis mothers. Mr. Sears concludes that the data (1) support Freud's claim that the commonest questions of young children concern the origin of babies, but (2) provide no evidence (*a*) that boys think that it is from injury that girls lack a penis, (*b*) that girls envy the penis, and (3) flatly disprove the universality of such reactions as (*a*) and (*b*) (p. 30).

It seems to me that the data do indeed make one regard with suspicion these claims of Freud's but that a much less crude method of verification would be required to disprove them. A boy's suspicion that a girl lacked a penis from injury might well not appear in his mother's answers to questions about what sex questions he asked, and a girl's penis-envy might also be very much hidden. I am all for getting clear about what technique would settle a question, and no one is more eager than I to ask an analyst what sort of discovery would make for his statement and especially what sort of discovery would make against it. But I do feel that, here and in other places, Mr. Sears in his desire to make the technique clear makes it over simple. No doubt the unconscious is irritatingly like ectoplasm—gone whenever the light is strong enough to see it. But still provided a psychologist's prescription for settling his claim is clear we must continue to be patient however complicated it is.

Chapter III is on Object Choice—the self as object, the oedipus situation, the latency period, homosexuality. Mr. Sears says (p. 57) "One conclusion stands out above all others: emotional development, as couched in terms of successive object choices, is far more variable than Freud supposed, but there are certain points at which Freud's skilled clinical observation provided generalizations that are largely supported by later and more objective observations".

Chapter V shows how the results of certain experiments with animals are connected in a most interesting way with Freud's theories of regression.

In other chapters, objective data are brought to bear also on some of Freud's theories about Distortions of Sexuality, Repression, Projection and Dreams. By the way, why is there "no aid to the interpretation of any particular dream in knowing that dreams are efforts at wish-fulfilment" (p. 132)?

In the concluding chapter Mr. Sears says something about whether psycho-analysis is or can become a *good* science and concludes that when the psycho-analytic "method is used for uncovering psychological facts that have objective validity it simply fails" and that "other methods must be sought for . . . the validation of scientific findings" (p. 133) although "social and psychological sciences

must gain as many hypotheses and intuitions as possible from psychoanalysis " (p. 143).

I should like to write at length upon this matter, but I must confine myself to saying that it seems to me that Mr. Sears, like most of us academic persons, is unduly dominated by a desire for cut and dried, very verbally manageable, evidence such as is to be had from crucial experiments or data from which correlation coefficients can be extracted. But the fact is that most convictions come from evidence the excellence of which cannot be so conveniently described, and surely convictions based on such evidence can yet be thoroughly rational. I should *like* to walk with Mr. Sears swiftly down the main street counting heads upon the side-walk, for I'm soon sick of playing games with no rules in the adjoining swamps and woods with Dr. Graham Howe and Mr. Layard. But I have an uneasy fear we'll be missing something and even that people may laugh at us in spite of our slide rules and statistical propriety. I am aware that the inclination to do science *a priori* and out of the 'inner consciousness' is still not dead and that we still need to attack every pretence of knowing about things or animals without watching them. A statement is a statement, I agree, only in so far as we all know what makes for it and what against it (tautology) and a statement provides new information in the way the engineer's, the chemist's, or the doctor's does, only if, like their statements, it better prepares us for aches and pains and sights and sounds. " The incidence of nail-biting is a function of age " does thus prepare us. But so does " Jealousy is cruel as the grave ",[1] though here no questionnaire will prove, much less disprove it, and we cannot present the evidence for it in a correlation co-efficient with its standard deviation. It is not that the ideas of questionnaire, experiment and correlation co-efficient are entirely out of place in connexion with it. They are not. But they can not be applied to it in the simple way they can to " Nail-biting is a function of Age "—though even here, as Mr. Sears remarks (p. 10, second paragraph), their application requires care.

Reading Mr. Sears I recall Dr. Graham Howe saying " Some still seem to think that there is no other truth but Science " (p. 6). I don't know what this means, but I happen to read now in a book before me " The remarkable thing about Mr. Flood is that he has bred hundreds of lion cubs and never lost one.

He will tell you that the secret is understanding lions, which does not carry you far. How do you understand a lion ? No two lions are alike, he will tell you. . . ." [2] At once we know that Mr. Flood does not think that " there is no other truth but Science ", and yet if a basis in years of careful observation makes opinions scientific

[1] So does Dr. Graham Howe's profound " Life exacts death as the price of its own renewal " (p. 241).

[2] *In Search of Ireland*, H. V. Morton, p. 34.

Mr. Flood's were scientific though not many of them were generalizations about all lions and the evidence for them was neither crucial experiment nor correlation co-efficients.

"No two lions are alike" he said. But doesn't this make science impossible? It's the opposite to "All lions are alike really, though of course some are bigger than others like motor cars." These two principles do not as they appear to do, make incompatible statements about lions, about nature. They express two opposite attitudes in the investigation of nature. But though the attitudes are opposite they are both needed in the scientist. The principles express two opposite calls for endless patience, to both of which the scientist must respond. For if when two things, apparently without reason, react differently he gives up looking for the reason why and says "They're just different", that is to fall into magic, to give up science and no longer look for the unity in nature. But also he must be prepared to find that he never comes to the end of the relevant differences, never gets in his list all the reasons—for otherwise he will be apt to call unscientific and unlogical those who still seek unity even in infinite variety and where the pattern of things cannot be presented in black and white on the forms provided by the logicians. We know that nature isn't always neat. We need to recognise that logic isn't either.

J. WISDOM.

VII.—NEW BOOKS.

General Biology and Philosophy of Organism. By RALPH STAYNER LILLIE. Chicago: University of Chicago Press; London: Cambridge University Press, 1945. Pp. 215.

The Directiveness of Organic Activities. By E. S. RUSSELL. London: Cambridge University Press, 1945. Pp. viii + 196. 8s. 6d.

THE first of these books is a full, careful but rather difficult philosophical discussion of the conception of the living organism by a leading American biologist. The second contains less discussion and is chiefly valuable for making accessible a number of the queer, almost incredible, facts which biological observers are constantly reporting and which theorists with ready-made explanations prefer to ignore.

One specially intriguing example given by Dr. Russell is the behaviour of a small freshwater flat-worm, *Microstoma*. It attacks the (for it) formidable coelenterate *Hydra*, not so much for its flesh as for the nematocysts, or stinging cells, *Hydra* possesses in its skin. Although the function of the nematocysts is to explode on contact and discharge their contents, *Microstoma* manages to swallow them intact. They are not digested, but absorbed whole and carried to the skin, where they are planted in an orderly way. These borrowed weapons then serve to protect *Microstoma* from its enemies. When *Microstoma* has obtained a normal complement of nematocysts it loses interest in *Hydra*, but, if very hungry, will eat one, digesting the rest of the flesh but spitting out the nematocysts. The American observers who studied *Microstoma* kept some out of contact with *Hydra* for several generations and then fed them with a special type (*Chlorohydra viridissima*) possessing four different kinds of nematocyst. *Microstoma* digested those which protrude adhesive threads and which would have been useless, keeping for use the two more ordinary kinds with poisoned darts. *Microstoma* possesses nothing that can be called a brain, and only the most rudimentary sense organs. Two further points should be mentioned in connection with this story. There are plenty of other species of freshwater flat-worm which do not borrow nematocysts and yet survive. There are quite unrelated marine organisms which have a similar habit of borrowing nematocysts for protection. As Dr. Russell says (p. 25) to interpret these facts we require the concepts of need, normal end-state or goal, and directive activity towards attainment of goal. In the account just given I have used, for sake of brevity, more anthropomorphic language which could have been avoided; but those three notions could not be dispensed with. The value of queer and unfamiliar examples like that of *Microstoma* is merely to shock one into seeing the factors involved. The very familiarity of familiar examples tends to conceal them.

Professor Lillie tries to make these factors clearer. His account is not easy to summarize, so that my attempt is stated in my terms not his, and I have also introduced what seems to me a slight improvement which he might reject. As will be immediately obvious, his theory owes much to the philosophy of Whitehead.

The natural world everywhere presents two aspects, constancy and change. Thus the constant velocity of light is a type of primordial or underived constancy; the constant reaction velocities of chemical compounds are types of consequent or derivative constancy, a residue remaining from previous change, which produced the compounds. In this second case, it is easy to see there is constancy of 'stuff' as well as constancy of relation. Older thinkers tended to take constancy of 'stuff' as primary and that of relation as secondary, for no very good reason. There is no 'stuff' at all in the more fundamental constant, the velocity of light, unless Space-Time is 'stuff'. Whatever may be thought of the notion of 'stuff' or of 'substance', the notion of 'structure' cannot be dispensed with; that is to say, the framework of constant relations within which occurs change or process, or function in the biological sense; and without which we should not find order in change. Most process is routine process, but, potentially at least, it is novelty, freedom, creativity.

The physical sciences are concerned with discovering and determining constancy, whether of structure or of routine in process. When inconstancy is found the physicist puts it aside as chance, randomness or entropy. Because of the nature of his purpose and method he does not take account of a third possibility, namely, directed, non-random inconstancy. Professor Lillie suggests (pp. 22, 80, 132) that the kind of inconstancy found at the sub-atomic level may be significant for biological processes. (Schroedinger does so too in *What Is Life?* 1944. But against the view, see I. Manton, "Comments on Chromosome Structure," *Nature*, Vol. 155, p. 471, 1945.) It is perhaps more likely that the smallest unit processes and structures of living things are too large for randomness at the level of the electron to be significant. If the smallest living unit involves a million electrons, their randomness or indeterminacy averages out. What almost certainly is significant is randomness at the molecular level, the random heat motion of atoms and molecules. For this reason, the analogy of human contrivances, like machines, seems to me even more valuable than it does to Professor Lillie. The analogy must not be pressed too far, however, because biological units are small relative to man-made structures, and show discontinuities of a kind not seen on the larger scale.

Consider a simple contrivance whose function is that of controlling or directing, an ordinary gas-tap. It consists of barrel and key, both rigid bodies; that is to say, within limits they are constant and determined in character. They wear out by friction in course of time; slow chemical changes may occur; the atoms composing the metal are in random heat motion; but all these things are negligible from the point of view of the tap's function over a reasonable period. Thus, each of the two metal parts may be taken as rigid and determined. In contrast, the relation between key and barrel is loose, inconstant, undetermined; from the point of view of strict physical theory, random. If the key fits so tight that it cannot be turned, the tap is useless; if it is so loose as to wobble, the tap is also useless; the looseness must be within limits. The looseness is quite clearly associated with random heat motions of atoms or molecules. The metal is rigid because heat motion is confined to oscillation about a centre. In the matter between the metal surfaces (air, moisture, grease and what-not) heat motion is not so confined; there is fluidity. There is some slight inter-atomic locking (static friction), so that a finite pressure is required to move the tap. When that pressure is exceeded and the tap moves, the molecular picture of the heat motions of the matter between

the metal surfaces changes from the purely random to flow in one direction or the other. After the tap is turned it becomes purely random again. All mechanically operating controlling devices depend upon the same principle, that a small imposed force temporarily turns random molecular heat motion into directional flow within a system otherwise rigid. The principle includes electro-magnetic contrivances controlled by switches, keys, microphones, photo-electric or chemical processes, as well as the more obvious levers, pistons, valves, ratchets, hammers, triggers, and so on.

The central nervous system of the higher animals is not entirely unlike man-made controlling devices. It contains one "rigid" part, the white matter, where nervous processes are insulated from one another and each follows a predetermined path. In the "loose" or "fluid" part, the grey matter, nervous processes are not insulated and the path probably not always predetermined. It may be guessed, however, that the controlling factor of directional flow is superimposed, not on purely random motion, but on some kind of 'fluid' order.

To return to the tap; there is nothing in its structure to determine if it is shut or open; it can be either. The physicist in devising his theoretical constructions may concern himself with what happens when the tap is open or when it is closed, but need not be interested in the opening or closing as such, nor why these acts are performed. The act of control can always be excluded from physical theory and must be excluded if theory is concerned with constancy. Inconstancy can always be treated as random process. The biologist has no need to submit himself to such limitations. It is part of his task to discover the constant conditions and routine processes of living organisms; and while he uses the experimental method it is bound to be these he sees most prominently. But if there is evidence of controlling factors at work he should acknowledge them. The acknowledgment, if properly made, does not contradict anything said about constant conditions; any more than the physicist contradicts his own physical theories when he turns on a gas tap in his laboratory. The trouble is, not to acknowledge the directive process, but how to deal with it when acknowledged. The old notions of 'animal spirits' and 'vital force' were obscurantist, if not self-contradictory, because they supposed a kind of matter not quite material or a kind of force not quite physical.

Professor Lillie adopts the simplest course of using the analogy of human purpose and initiative. It has the advantage that we are acquainted to some extent with the working of our own purposes. It has the disadvantage that it may lead to our reading into the processes of lower organisms characters quite alien to them, belonging solely to our own mental life; in short, it may lead to the vicious sort of anthropomorphism. Before condemning it for this, it is well to consider the alternatives. First, there is the theory that living organisms are no more than complex mechanical systems, and that chance and natural selection have brought them to their present condition. As against this theory: if everything anthropomorphic is to be condemned the notion of chance will hardly bear scrutiny, unless it just stands for ignorance. In that case, the theory is not really a theory. As to natural selection: it explains very well how unsuitable types have perished but hardly explains how the survivors have come to be as they are. In the case of *Microstoma*, just quoted, only a completed co-ordinated process confers an advantage; single parts of it are no help. It is no use *Microstoma* swallowing nematocysts unless they are conveyed to the right place. Unless they are being swallowed, how can arrangements for

conveying them develop? Of course, the supporter of chance and natural selection gets over the difficulties by inventing *ad hoc* hypotheses to explain what happens at each stage; a method that will save the worst possible theory.

Another alternative which seems more promising uses the Aristotelian notion of a hierarchy of forms. The lowest stage is the inorganic realm where chance and mechanical causes operate alone. Above that is the vegetative realm where a certain order is imposed on what would otherwise be chaotic, namely, the minimum required for growth and reproduction of the lower organisms. The order is teleological, because growth and reproduction are processes directed towards ends, but it is limited or minimum teleology. Above the vegetative realm are higher ones where the vegetative order still operates but where there are superimposed higher orders required for higher animal functions. Finally, there is the highest known order, that required for intelligence. This kind of theory is not to be dismissed lightly. It suffers, however, from a conspicuous weakness, probably shared by any alternative theory not purely mechanical. When we ask what in fact constitutes the order or form of the vegetative realm or any below the highest one, the answer is given in terms derived from human experience and purpose. Or else it is a worse answer in pseudo-mechanical, pseudo-psychological terms, like 'vital force'; or else it falls back on mechanics, chance and natural selection. In short, Aristotelian theory is not so much an alternative theory as a special, more complicated version of Professor Lillie's.

Perhaps then, we have just to treat the formative, ordering or directive factor as if it were psychic and take the analogy of human contrivances seriously. There is an interesting paradox here. Men most conspicuously exercise their freedom of choice in designing, making and using machines. Every experiment, measurement or controlled observation in science is both an act of choice and the use of a machine. Yet it is from contemplating machines and the consequences of scientific observation that men have arrived at the notion of mechanical causation and have developed deterministic theories denying human freedom. This shows that, as Kant put it, mechanism and teleology are incompatible if taken as constitutive principles; while they are complementary if taken (as they should be) as regulative merely.

This still leaves the last and most difficult question, as to which Professor Lillie, following Whitehead, does no more than provide a useful hint (pp. 107, 191, 205-207). Physical science is cognisant of a temporal order of past events which is taken to be permanent, but is not cognisant of the transient present of consciousness. This transient factor is one element of human consciousness, which contains also an element of the permanent; so that the mind may be said to be the cross-section of the two. Let us suppose that every living thing is at a similar cross-section. The special human privilege is to have a sort of double view. In action the transient is experienced as such, and as present, but it is contemplated as though past. The permanent in conscious recollection is taken as though present; it also forms the habitual basis of action. At lower levels there may be a single view only and yet a cross-section. This account does not solve the problem, but may help to avoid the fallacy of reading into lower life-processes factors which are not there, while admitting directiveness and dealing with it in terms borrowed from psychology.

A. D. RITCHIE.

The Criticism of Experience. By D. J. B. HAWKINS. London: Sheed and Ward, Ltd., 1945. Pp. ix + 124. 5s.

A COMPLETE theory of knowledge, according to Father Hawkins, comprises two groups of questions. First, the strictly philosophical part of logic, that is, the nature of thinking and of judgment, the validity of universals, the scope of implication and of reasoning. For this, a foundation of formal logic is required and could with advantage be learned in school. The second group of questions is about what sort of things we are able to know and how we come to know them. Here we test the credentials of the kind of knowledge which common sense takes for granted—we try to make a systematic criticism of experience. Father Hawkins' book deals with this second group of questions and is intended as a text-book for Roman Catholic institutions of higher education. It is, of course, clear that the author believes that philosophy can do much more than criticise experience: he holds that such a criticism is a foundation for the rest. We have then an enquiry about how we can know material objects and other minds and what we can know about ourselves. And these familiar questions are tackled along the familiar lines.

The author first decides that there is nothing in the nature of knowing to confine us to the knowledge of the self: knowing is, "of its nature, a way of escape from subjectivity, from the fatality by which you are merely what you yourself are. You cannot *be* more than you are, but you can *know* things other than yourself" (p. 12). Nevertheless, reflection shows us that in mere sensation we do not in fact transcend the self: sense-data are (of course) unequivocally *real*, but they are subjective—states of the self and no more. They are not given as copies of anything else, and if they were the only objects of direct acquaintance, we should at best only be able to *infer causally* the existence of other things in the world beside the self. It is true that a world could be constructed out of the sense-data themselves, but it would be a subjective world. Now the causal inference to an objective world could not make sense if there were *nothing* of which we had direct non-inferential knowledge: for there would then be nothing against which our inferences could be tested. "Much, doubtless, of our knowledge of the external world is inferential, but it can scarcely all be inferential" (93). The author therefore argues that we can and do have direct acquaintance with objects other than the self, and that this knowledge is non-sensuous. By a careful examination of the "conditions of perception" it is shown that we are aware of external things as agencies which (together with the mind itself) produce sensations in us. Hence we are directly aware (1) of the states of our own mind (which include sense-data); (2) of independent external objects; and (3) of a causal connexion between these two.

The general plan of the argument used to establish these conclusions, is to exhibit the limitations of mere sensation, one by one. We are asked to recognise that sensation alone cannot explain our notion of three-dimensional space, our notion of duration, our notion of the unity of the self, our knowledge of past time, and our knowledge of other extended bodies and of other minds. It must not be inferred that the book is written round a negative thesis: on the contrary, the point of the book is that on all these subjects we do have genuine knowledge; that, although some of our judgments on these subjects are dubious, they cannot all be dubious and they cannot all be inferential.

With regard to space, Father Hawkins rejects the view that our notion is derived from the data of sight and touch. Sight gives us only two dimensions: the data of touch are seen, on reflection, not to be extensive at all. We have, however, accompanying inner organic sensations, an intuition of the mass and volume of different parts of the body—an extension in three dimensions whose parts and limits cannot be precisely known without the aid of *symbols* from the properties of things seen. The differences between this primitive consciousness and the visual symbolism of two-dimensional space, has somehow to account for mistakes in localisation (55). In the last chapter of the book it is seen that this intuitive knowledge of our own body is continuous with a direct awareness of other bodies. For it is when there is contact between my body and others—when the mass of another body is compressing the mass of my own body—that I have direct acquaintance with external things. In such a situation I know that there is a real and independently existing thing there, and that it is acting on me to produce (in co-operation with the agency of the self) the sensation of pressure of which I am aware.

In a parallel way, it is argued that in mere sensation we have knowledge only of what is present, but that where we have sensations continuing unchanged and realise that there might have been a change, there we have an intuition of duration. This is somehow the origin of our complex notion of time: presumably the "symbols" used are motion and the measurement of distances in two-dimensional space. Again, a later chapter shows that this intuition is continuous with an intuition of *past* time: it is, the author argues, impossible that all instances of memory should be inferential; we must, therefore, know directly both that there was a past, to which our memories (true or false) relate, and that in certain cases that past was the primary agent in producing the present memory. There must be direct memory-knowledge if only to provide a test for memory-inferences.

The argument for the direct awareness of the self as substance follows a somewhat different line. First of all, if anything at all is real, sense-data are real; but since (it has been argued) these must be subjective, we can at least say that we have direct awareness of the successive states of the self. But does introspection discover nothing but the thinking itself? Father Hawkins treats this question historically, showing how the merely logical subject gets confused with a merely linguistic device and gradually evaporates before the criticisms of Hume and others. For Hume, then, the whole world is composed of impressions and ideas, "objects which are very like universals in so far as they do not seem to contain any factor incapable of description in universal terms" (81). What is lacking is a principle of individual existence, and Father Hawkins shows that the original Aristotelian doctrine of substance is one which has real content: substance is a power, an appetite, a principle of agency. "It remains to be seen whether this notion of substance can be justified by experience and reflection".

Reflection shows that it can. I know myself as an active individual, not only in voluntary effort but in every sort of experience. Moreover, it is not the mind, but the embodied self, that is the substance. This awareness of the self as substance is of course closely connected with an awareness of other bodies as real and active individuals: both are intuited at once in perception: "The way in which one thing may in a certain sense be present in another is by causal determination. Awareness, being of its nature capable of transcending the subject, finds the possibility

of intuition in the causal presence of other things" (110). Here the notion of *causality* is brought in to help us through the difficulties of the notion of *knowing*. The author also holds (but does not argue in detail) that we can sometimes have direct, non-inferential knowledge of other embodied selves.

It might seem at first reading, as though these arguments made an extraordinary discrimination in favour of certain internal bodily sensations, as against the sensations of sight, hearing, etc. It might seem that the author was prepared to trust unreservedly in the stresses and strains of his muscles but to dismiss the data of the five senses as never anything but subjective. But this is not what Father Hawkins really says. There are, he says, certain bodily feelings of strain, effort, resistance, interplay and involvement of the several parts, and these feelings are invariably *accompanied by* an intuitive knowledge. And it is important to notice that he holds (with Whitehead and others) that the bodily feelings of strain and effort, are themselves invariably conjoined with sight, hearing, taste, touch and smell. For they occur when the body is doing or suffering in any way whatever: though presumably they are usually too faint to be introspected without very special attention. So that the intuition is closely connected both with internal feelings of the body, and with the activity of the peripheral senses: but it is not itself a sensation or feeling of any kind. What we intuit, is the existence of things occupying neutral space and time, things having force, mass, "intrinsic motion", things acting upon the embodied self, either now or in the past.

There is, I believe, another important distinction to be made if the main thesis of the book is not to be misunderstood. The intuition is not supposed to be a mere *knowledge that*—as, for instance, I might have a direct intuition *that q follows from p*. I do indeed also have the *knowledge that* there is an external object acting upon me; and it is just because (in the author's view) nothing that I see, or touch, or hear, enables me to have this "knowledge that", that he claims that I must have some kind of *awareness* that does not come to me through the senses. While it arises vividly along with certain bodily sensations, it is not itself sensational at all. But these distinctions are not made in the book, except by implication. Obviously they raise the whole question as to the exact sense in which we can say that acquaintance (through the senses or otherwise) is *knowledge*. And this in turn draws attention at once to the part played by language—to the questions and answers that do in fact occur in real intercourse, about "I see" and "I know". I suspect that if one began in this way, the central problems of the book would soon take on a very different aspect.

The test-problem for a theory of this kind, must be to explain the parts played by intuition and by inference, in our knowledge of the external world. For our direct intuitive knowledge of extended things must be of very modest dimensions. As I sit here, for instance, I know that something is acting on my eyes to produce the mosaic of colours which makes the two-dimensional whole of my present visual-field; and I *recollect* something of the way in which I have been at various times in contact with objects in the room. But (as Father Hawkins writes): "I have a genuinely intuitive perception of the chair in which I am sitting"—which suggests that justice has not been done to the cognitive powers of that part of the anatomy which occupies the seats of chairs!

Father Hawkins does not attempt to explain in detail how we come to make inferences by the use of sense-data. The relation of "pressure-data" to the data of the five senses is obscure. In particular, it is hard to

see how, on this theory, I can ever *know* that an external agent is concerned with this or that particular datum of one or other of the remote senses. Granted I may know, when I press my hand on the table, that the object is the cause of the feeling of constriction in my hand: how can *I know* that it also causes the shape and the colour that I now see? The author speaks of using sense-data as signs or symbols: this suggests Whitehead's principle, "the intersection of the two modes of perception": but though Father Hawkins quotes from *Symbolism* with approval, he makes no attempt to introduce the more subtle notions of Whitehead's analysis.

This account should make clear to the reader, whether or not this little book will bring grist to his mill. It should be mentioned that the argument moves briskly forward along a wide front and that the impression is given that every one of the problems in the field is somehow brought to a satisfactory conclusion. This, of course, cannot be: in fact, many of the discussions are much too brief and superficial. Nevertheless, something well worth while is accomplished. Inside the trim boundaries of the familiar themes, the author inserts an acute discussion of two fundamental weaknesses of British Empiricism: first, it seems to oblige us to say that *all* our judgments about the external world are doubtful; second, it seems to oblige us to try to describe a world of individuals in terms that are all universal or general. At least Father Hawkins makes the reader see the importance and the difficulty of these two problems, and enables the reader to watch somebody trying very hard to do an important job. It is partly for this reason that the book could be recommended for use in an elementary class.

There is another good reason for recommending its use in institutions of higher education (whether Catholic or otherwise). The discussion of the various topics, though not attempting to give their long history, makes excellent use of the history of philosophy and provides an introduction to important controversies of the past. The author has some talent for the broad history of ideas, and moves easily from the Greeks to the British Empiricists, and from Descartes back to Suarez and S. Thomas. His professional studies no doubt gave him a particular interest in the scholastics. While he makes it clear that it was Descartes who founded the criticism of experience as an independent and (would-be) rigorous branch of philosophy, he insists rightly, that the less deliberate and less systematic views of earlier philosophers also have great value. His account of the Representationalist controversy, from Democritus to Hume, is one example of good broad exposition. Amongst the moderns, the author gives a special place to Reid and Hamilton.

KARL BRITTON.

A Preface to Logic. By MORRIS R. COHEN. New York: Henry Holt and Company, 1944. Pp. xi + 209. $2.50.

IN this collection of studies, the late Professor Cohen examines some of the border-line problems of logic and philosophy. He explains in his Foreword that the book is not intended to be another treatise on logic but an exploration of such peripheral problems as 'the relation of logic to the rest of the universe, the philosophical pre-suppositions which give logic its meaning and the applications which give it importance' (p. xi). The work contains nine studies of such problems which include the subject matter

of logic, the nature of propositions and the relation between logic and language, probability, statistics and scientific method, and, finally, the contribution of logic to the study of values and the elucidation of general world order. An appendix contains reviews of F. H. Bradley's *Principles of Logic* and John Dewey's *Essays in Experimental Logic* written in 1927 and 1916 respectively, in which Professor Cohen gives his estimate of these logicians and their contributions to the subject.

It is impossible for a reviewer to chase all the lively hares started by these discussions. Professor Cohen is stimulating on all the perennial logical puzzles, and those interested in them can be confidently recommended to his pages. Throughout, however, he defends a realistic or ontologic view of logic. He is slightly contemptuous of certain recent doctrines of the meaningless or verbal character of logically necessary propositions. For, 'I cannot see', he says, 'how logical and mathematical statements, if they are characterised as devoid of sense or meaning, can also be true or false' (p. 48). And that the propositions of logic are true is indisputable. He is not, perhaps, quite fair to the logical positivists who use 'meaningless' in a special sense. To say that logical propositions are 'meaningless', in this sense, signifies only that they are analytic or tautologous and, particularly, that no empirical facts are relevant to their truth or falsity. And Professor Cohen himself would agree that logically necessary propositions are not verified or disproved by observable situations. Nor do Carnap and his followers deny that they are true *in some sense*. By interpreting them as tautologies, rules of grammar or conventions, they strive to determine the particular sense in which they are true. As Professor Cohen rightly points out, however, logic is not what is ordinarily meant by the 'grammar' of English, French, German or any other particular language, but is common to all methods of general communication. Even if the positivists were right, therefore, the problem would still remain of determining the special language of which logic is the grammar.

I find Profesor Cohen's own alternative solution, however, somewhat befogged by the blessed word *possibility*. The propositions of logic (with which Professor Cohen identifies mathematics) are necessarily true. What makes them so? he asks. Not feelings of certainty, linguistic rules, or empirical facts. But facts of some sort. 'The existence of the logical or relational structure of Euclid's geometry is as much a fact as the composition of albumen, the structure of rocks, or the construction of the solar system, all of which depend on geometric relations' (p. 15). But the laws of the composition of physical bodies or systems state facts about the observable constituents of those complex objects. What are the objects connected by geometrical or logical relations? Professor Cohen tries, as follows, to explain the difference. The empirical sciences assert laws the contraries of which are abstractly possible. Logical laws, however, are those whose contraries are devoid of meaning or application to any possible determinate objects (p. 16). They are, therefore, those which express 'the most generally or abstractly possible relations between *all*[1] objects' (p. 44) and this field of possibilities explored by logic represents 'the ways in which the *elements of the actual world* can be arranged and transformed' (p. 56).

Later on, possibility is distinguished as abstract and concrete (p. 180). Concrete possibilities are expressed by those hypothetical propositions

[1] My italics.

which state what will occur to an existing object under given empirical conditions. 'If this is butter, it will melt in the sun'. Bare or abstract possibility is the class of all possible objects and their transformations (p. 180). Hence, logic 'is an exploration of the field of possibility just as truly as astronomy is an exploration of the field of stellar motions' (p. 181). So that, according to Professor Cohen, to say that the laws of logic are necessarily true is to say that they apply to all possible objects and not only to those which happen to exist, But this is, surely, fallacious? It is true that 'if p is included in r and r is included in q then p is included in q', applies to any class of objects which does or might exist. To say that this proposition is logically necessary, however, is to say that the conclusion is entailed by the premisses and that its contradictory is self-contradictory. The application of the proposition to any suitably defined set of objects is an independent logical operation. It cannot, therefore, be the ground or reference of the logical necessity of the original inference. There is no magic in imaginary facts (and what more are possible facts or possibilities?) which makes propositions about them necessarily true while those about actual facts are contingent. Moreover, the law of contradiction is logically necessary and, in one sense, its application is thereby impossible. For there could be no proposition which was at once both true and false. That logical propositions apply to possible objects cannot, therefore, give an *explanation* of their distinguishing characteristic of being *necessarily* true. Nor, in my opinion, is the search for such explanation a genuine investigation. There is nothing more fundamental or better known by which logical necessity can be explained or interpreted.

On probability, Professor Cohen defends, in general, the frequency theory, maintaining that most judgments of probability are either founded upon, or correlated with, statistical evidence. (p. 120). In a brief paragraph on p. 121, however, he does admit that other tests of reliability may be admitted into the establishment of scientific conclusions. He does not, however, enlarge on these and one is left with the impression that they are relatively unimportant. But this seems doubtful. The probability of a scientific theory, for example, is surely, not derived from the number of times that theories based on evidence obtained by similar methods have been established. In estimating the evidence for the probability of Freud's Theory of the Interpretation of Dreams, an investigator does not count the Electronic Theory of Matter, the Mendelian Theory of Inheritance, Keynes' Theory of Money and every other established scientific theory as evidence of its probability, but only the particular propositions which Freud himself adduces as evidence for his conclusions. Relative frequency is most clearly applied to the interpretation of the probability attaching to propositions about further occurrences of a class of relatively homogeneous, repeatable events, such as the yearly number of suicides, the proportion of passes and failures in next year's matriculation examinations, etc. Only by an unwarranted stretching of the meaning of the word 'frequency' can it be applied to the partial confirmation of all empirical conclusions. But there is no point in thus misusing a perfectly good word instead of recognising different senses of 'probable' appropriate to the types of evidence accepted by qualified investigators for the establishment of conclusions in various branches of empirical knowledge. Nor can a theory of probability be adequate which makes nonsense of such a statement as 'I shall probably go to London to-morrow' except by reference to previous journeys entirely irrelevant to to-morrow's mission.

In a study of 'Values, Norms and Science', Professor Cohen ascribes to ethics the task of clarifying and systematizing the reasons for human preferences which, when formulated, are 'norms'. Nevertheless, 'norms' are also *good* reasons for preferences and not merely any by which a preference happens to be supported. Unfortunately, Professor Cohen is not very enlightening on the criteria of good reasons or 'norms'. He rejects the positivist or relativist view that since moral values vary in different societies you may 'pay your money and take your choice'. And he is right. Some choices *are* better than others. But not, he thinks, by reference to any absolute standards of moral worth. He contents himself with the somewhat vague assertion that reasonable choices are possible if moral principles are treated as incomplete symbols to be completed by the particular circumstances of their application. 'Lying is wrong' and the fact that Spartan boys and Hitler youth were taught to lie does not make it right, as the relativist view sometimes suggests. But neither is 'Thou shalt not lie' an absolute law to be observed in all circumstances. If a greater good would be achieved by lying it would be right to lie. But beyond reminding us to use our wits in moral conduct, this is not very helpful.

The book is completed with an index. There are misprints on pages 68 and 97. M. MACDONALD.

The Development of Mathematical Logic and of Logical Positivism in Poland Between the Two Wars. By Z. JORDAN. (*Polish Science and Learning*, No. 6, April 1945.) Oxford University Press. Pp. 7-39. Bibliography and Annotations, pp. 40-47.

This issue of *Polish Science and Learning*, as contrasted with those previously published, is devoted to a single essay. In it Dr. Jordan gives a survey of the contributions of Polish writers in mathematical logic and an account of logical positivism as developed in Poland. Written under most difficult conditions, while the author served in the Polish armed forces, it nevertheless contains an amount of detail surprising in light of the writer's lack of access to Polish books and articles referred to, and a most useful and extensive bibliography.

The names of Lukasiewicz, Lesniewski, and Tarski, who led the Lwow-Warsaw School, and of Chwistek at Cracow, are familiar to English-speaking readers. Rather than summarise the monograph's ten sections, the reviewer will try to group around these figures and a few others the chief contributions. *Principia Mathematica* was the starting point for two main directions of work: I., attempts to use logic as a means to solving foundation problems of mathematics; II., study of the construction and properties of formalised deductive systems.

I (*a*) Chwistek's discovery that on the basis of Russell's theory of types Richard's antinomy could be reinstated by use of the axiom of reducibility led him to his "theory of constructive types" which dispenses with this axiom. To meet the criticism that a great deal of mathematics was thereby sacrificed, since his theory made it impossible to prove the existence of the continuum and thus curtailed the Cantorean theory of sets, he showed that analysis could be reconstructed on the basis of his system. Dr. Jordan's exposition of Chwistek's system is the least clear of the discussions in the survey. He characterises it as motivated by opposition

to "verbal" philosophy, which in the context suggests opposition to the assumption of non-constructible objects; but his account of Chwistek's conception of semantics as "the set of rules and formulæ by means of which expressions in logic and mathematics can be constructed mechanically" (26) leaves both this opposition and the conception of semantics unclear. (*b*) The attempt to provide a foundation for mathematics in logic led Lesniewski to formulate a system consisting of "Protothetic" (the propositional calculus), "Ontology" (roughly, the theory of classes and relations), and "Mereology" (which is formally similar to the Boolean algebra except for the exclusion of the null class). The aim of the latter was to establish a theory of sets free from the paradoxes. In it "semantical categories" are to replace logical types. (*c*) Kuratowski reduced relation theory to class theory (*cf.* the Wiener-Kuratowski definition of ordered couples in terms of classes). And he used the logical notation in solving the mathematical problem of evaluating a projective class of a set of points and in proving theorems found by more complicated means. (*d*) On applying a logical procedure to defining a mathematical notion (*e.g.*, "the definability of a set of real numbers") Tarski was able to use "structural properties of the definition to draw inferences about the properties of the defined notion". (20)

II. Study of formalised deductive systems issued in (*a*) new developments in connexion with propositional calculi and (*b*) metalogic. Among the contributions falling under (*a*) Dr. Jordan mentions the discovery by Lukasiewicz (simultaneously with P. Bernays) that the *Principia* primitives are not independent, and his construction of a complete set of three primitives, using the CN symbolism which he devised to dispense with dots and brackets; several alternative reductions of the primitives for the propositional calculus to one primitive only (Lukasiewicz, Wajsberg, Lesniewski, Tarski); discovery of formulæ equivalent to the Nicod-Sheffer axiom, and simplification of it (Lukasiewicz, Tarski, Wajsberg, Sobocinski); introduction (1920) of truth-tables by Lukasiewicz[1]; formulation of the necessary and sufficient conditions which a function "*f*" with a propositional argument must satisfy in order to be a truth-function in Whitehead-Russell's sense (Tarski); Wajsberg's proof that the system of Strict Implication is not reducible to that of Material Implication; and finally, Lukasiewicz's discovery of many-valued systems of logic,[2] which is judged by Dr. Jordan to eclipse everything done in the field of logical research in Poland. His three-valued system, which is motivated by the idea that statements about the future are neither true nor false, contains functions partially coinciding with modal functions. *n*-valued systems beyond $n = 3$ are interpreted with difficulty if at all.

(*b*) It was natural that there should develop a wide study of general features of formalised deductive theories, and that the distinction between logic and metalogic, between expressions of a system and the language we use in speaking about these, should arise. According to Tarski it is imperative that this distinction be observed since ordinary speech, which blurs it, gives rise to antinomies. The conviction that ordinary language, which provides the metalogical, or syntactical, terms of a system, is an

[1] The idea of truth-tables was developed at least as early as 1902 by Peirce.—Reviewer's Note.

[2] Lukasiewicz's three-valued system appeared in 1920, his *n*-valued system not until 1930. E. L. Post's *n*-valued system appeared in 1921. —Reviewer's Note.

inadequate tool, resulted in careful scrutiny of such terms as "axiom", "conclusion", "rule of transformation", "proof", "primitive term", "definition". According to Lesniewski the use of definitions (which he contends are not mere convenient abbreviations) requires special formative rules, and the rules of transformation a series of "terminological explanations" (forty-nine such are given). Tarski showed that the terms "deductive system", "set of axioms", "independence", "completeness", "consistency" could all be defined in terms of the notions of a meaningful statement and of the consequence relation. (*E.g.*, a set of propositions is consistent if it is not equivalent to the class of meaningful statements.) These two notions were taken as primitive in Tarski's Calculus of Systems, in which the variables represent deductive systems. This calculus constitutes a partial interpretation of Boolean algebra. It is formally similar to Heyting's propositional calculus, inasmuch as only a weakened form of the law of excluded middle holds. ($\overline{\overline{X}} = \overline{\overline{\overline{X}}}$ is a theorem, but $X = \overline{\overline{X}}$ is not.) Finally, metalogic itself was axiomatised by Tarski and in it the notion of proof made precise. The reviewer found this account of Tarski's metalogical system not as clear as could be desired.

Following the sections on mathematical logic and a short section on work in the history of logic are three sections on logical positivism in Poland, the development and representatives of which are largely unfamiliar to English-speaking readers. One had hoped that the section on its distinctive features, which the author insists it has, would have marked it off more clearly from the work of the Viennese Circle. These features are set out as follows: "ordinary speech is an inadequate tool in philosophical research; so far as philosophy is concerned, language and not introspection and intuition is the main source of evidence; the proper subject matter of philosophy . . . is the fundamental notions and propositions as well as the methods of particular sciences . . . only verifiable propositions are scientifically valid". (32)

The outstanding figures of logical positivism in Poland are Kotarbinski and Ajdukiewicz. Although amongst Polish positivists "there is no pronounced tendency to form a system" (32), Kotarbinski did construct a system, called "Reism", whose main theses are stated to be: the category of things (physical objects) is "the only ontological category" (34), names for these alone being proper subjects of subject-predicate sentences; there are no sensory qualities (*cf.* New Realism); psychological statements describe objects of the external world; psychological and physiological processes are identical. The affinity of this view with Physicalism is clear, although Kotarbinski did not go so far as to attempt an "Einheitswissenschaft", or "system comprehending all scientific statements formulated in a constructed homogeneous language". (34) Opposition to logical positivism in Poland took much the same form as elsewhere.

Ajdukiewicz's work consists mainly in the investigation of language (but not, according to Dr. Jordan, of any of the "natural" languages). The meaning of an expression in a language is, roughly speaking, defined in terms of rules for its usage. Ajdukiewicz's view is that any language consists not only of vocabulary and rules of syntax but also of rules of meaning ("rules establishing the correspondence between a word or an expression and a meaning. . . ." (35)). Such rules are of three kinds: axiomatic rules (*e.g.*, $1 + 1 = 2$); deductive rules, requiring the acceptance of conclusion from premises; empirical rules (to quote the unclear illustration given (35): "if John understands 'P' and 'P' = 'John

experiences so-and-so', then John cannot deny 'P' when he experiences so-and-so"). Various languages exist, i.e., various systems of meanings, which are equally valid. (*Cf.* Poincaré's conventionalism.)

The monograph closes with a personal postscript giving the historical setting of adversity and disruption in which the work of the generations between the two wars was carried on.

ALICE AMBROSE.

The Rediscovery of Belief. By Louis Arnaud Reid, D. Litt. London: The Lindsey Press. Pp. 203. 6s.

THE aim of this book is to bring out the limitations of humanism and to show how a person of enlightenment should view religious questions. Beginning with a shrewd diagnosis of the main ills of our time, it proceeds to a number of important topics ranging from education and social questions to the authority of the scriptures (a timely plea is made for greater dissemination of the established facts about the historicity of the gospels), the use of the creeds in worship (by entering imaginatively into the experiences they embody while "keeping our hardly-won freedom and integrity"—p. 178), the 'person of Jesus' and the doctrine of the Atonement. The discussion does not always carry conviction but it is informative, suggestive, and concise. Professor Reid has packed a great deal of philosophical wisdom into his book without making it difficult for the layman or tedious for the expert—even when discussing the well-worn theme of Hedonism. The book deserves to be widely read.

It is the author's account of humanism that most invites comment here. It is contended that the humanist fails especially to deal with moral evil. This is for two reasons, not always very clearly distinguished by the author. (*a*) Even if the humanist does not seek "to explain evil away, in terms of psychology or history or social environment or lack of education" (p. 66), he cannot in consistency acknowledge "the radical nature of moral evil" (p. 67), for he is bound to be "measuring evil by a human standard which he himself has provided" (p. 69). Standards which "transcend man and are given to him and not made by him" must have "an implicitly religious basis" (p. 68). But there the reviewer must confess to much difficulty. Is there not a third view which Professor Reid does not discuss? May we not avoid relativism ("the shallowest of doctrines" p. 44) in terms of objective ethical standards intuitively discerned and not derived at all from the nature of man himself, without raising any distinctively religious questions? The case for moral objectivity has in fact, been most persuasively presented without recourse to religious matters, and Professor Reid himself fully admits that "genuine morality exists quite independently of any religious beliefs" (p. 99), that there is "a genuine intrinsically authoritative morality" (p. 91). But he holds that the moral standard, none-the-less "depends upon what (one sort of) religion affirms" (p. 92), "human good would not command were it not for God, because it would not exist" (p. 103). But is this dependence peculiar to ethics? Is more affirmed than that, for a religious view, everything stands in some dependence upon God? If not, we can answer the relativist without recourse to religion. But the suggestion seems to be that we cannot. If so, we need to be told very carefully why—as we are not told by the author. His procedure seems to be to present the case for religion in general, it being assumed that this carries with it his own view of the

dependence of ethics on religion. But it may well be argued that we need to answer the relativist as a step in the process of building up a religious view : that, among other regrettable consequences, too direct an absorption of ethics in religion is very harmful to the cause of religion (it would still be true that religious views would amplify the content of our moral ideals in important ways). (*b*) Man cannot of himself conform to transcendent standards—he needs the help of God. This again is very perplexing, and not less so because very familiar in religious thought. To say that the standard " is not concocted or set up by man himself " (p. 64) is one thing, to set it beyond his reach is another. Transcendence in the first of these senses could be ensured by ordinary objectivist ethics ; how is the second obtained, and how can an ideal be an ideal *for us* and a measure of *our evil* if man cannot fulfil it ' by his unaided effort ' ? Professor Reid follows the orthodox doctrine in denying that " evil is eliminable by man " (p. 67), but he does so with some hesitation, for he declares also, that " there is no absolute inevitability about sin " (p. 174) although sin " is almost infinitely likely to happen " (a hard statement) and that, on the other hand, Jesus was sinless but not bound to be so—our estimate of him being " empirical " (pp. 172-174). But in the main, he seems assured that there is " a corrupting element in all men's actions ", that " all love of man for man is ultimately the gift of God " (p. 102), that the unaided effort of man to be rid of his sin " in itself implies separation from God, a form of egoism, and therefore sin " (p. 157). He also describes sin as " subjective blindness " (p. 96) to be cured by a new vision of God, but the blindness betokens moral evil and guilt, man is wicked because of it. For " the will is free " and " the choice is finally man's " although " the victory . . . is of God " (p. 123). But this appears hardly consistent. Nor will it help to ascribe ' the sin of unbelief ' to the fact that " man will not see the truth " (p. 122), for this ambiguous statement is acceptable, if at all, only in a highly elliptical sense. We can only will to do (or not do) what will make us see. It does not seem, therefore, that Professor Reid has succeeded in making the traditional doctrine more coherent, and we simply cannot make a virtue of sheer contradiction by calling it a paradox. It seems plain that sin, moral evil, and guilt, can only refer to what is avoidable by the individual. If the ' sense of sin ' has significance beyond this, and it may well refer to *something* important in religious experience, it cannot be to *sin* that it really points. Is anything to be gained by continuing to deny this in the face of elementary ethical principles ? Is not the idea of original sin irretrievably barbarous, and would it not be the better part of valour to seek a new conception of the relation of ethics to religion ? The problem at least cries out for discussion, especially in view of the present state of theology, and it would have been helpful if Professor Reid had attacked it more boldly.

This bears on two other main contentions of Professor Reid—firstly, the view that the wicked put themselves " outside the pale of ordinary human consideration " (p. 81) because the ' charity of humanism ' is limited to ' selfless affection ' based on ' lovable qualities ' of character, and, secondly, the advocacy of the ' double standard ' and the ' special vocation ' of a few to live by an ' impossible ' ' perfectionist ' ethic which is also an ideal for the majority in so far as it is directed " not to actions at all, but to the very disposition of the soul itself " (p. 140). (It is held, not, I think, on very solid grounds, that Jesus himself did not expect his injunctions to be generally followed but was " as far as this world is concerned a real

defeatist in his teaching "—p. 140). The position as a whole would be improved if it were presented frankly as a Socratic ethic (much derided as advanced by the humanists—*cf.* p. 58) without infusion into it of incompatible ideas of guilt and responsibility. Its difficulties point to a completer revision of accepted religious doctrines than the author is prepared to undertake.

H. D. LEWIS.

Berkeley's Immaterialism. By A. A. LUCE, M.C., D.D., Litt.D. London: Thos. Nelson & Sons, Ltd., 1945. Pp. xii + 163. 6s.

THIS is a captivating book. Written in a manner which recalls the fresh and racy idiom of the *Principles*, and presenting its case with something of the same disarming directness, terseness and naïveté, it bids fair to become a popular text-book with those who desire a more sympathetic and less disparaging treatment of Berkeley than is usually accorded to him. It is by its charm, however, rather than by the weight or cogency of its arguments, that it must hope to make converts to the immaterialist faith.

The book is divided into twelve short chapters, with an Introduction, in the form of a brief biography, and an Appendix, in which the discussion of immaterialism is brought up-to-date. In chapters III to VI the first seven sections of the *Principles*, which "contain the essence of Berkeley's case", are expounded *seriatim* with considerable clarity and felicity of expression but with little critical appreciation of the difficulties involved. Berkeley's famous doctrine of the Divine Cause is discussed in chapter IX and his replies to Thirteen Objections in chapter XI. I have, regrettably, no space, in a short review, in which to summarize the contents of these chapters. I can only indicate briefly my main reason for feeling dissatisfied with the book as a whole.

It seems to me, that Dr. Luce sets out to do what simply cannot be done, which is to convince the critical reader that "Berkeley's immaterialism is sound common-sense" (p. vi). He builds his case on the claim that the denial of matter in Berkeley does not imply a theory of 'private worlds'. "Objectivity is the key-note of the *Principles*. He [Berkeley] is no solipsist, nor subjectivist, nor subjective idealist" (p. 46). Notwithstanding his insistence on the intra-mentality of objects, he keeps "what is in the mind 'intirely distinct' from the mind itself" (p. 51), and "is always loyal to the subject-object relation" (p. 28). He even "affirms external things in the ordinary sense of . . . things other than ourselves, things of external origin". (p. 136). The crux comes, however, when the question arises as to the objectivity of what lies outside the range of actual perception at any moment. Dr. Luce is at pains to show that Berkeley "has room for the perceivable as well as the perceived" (p. 46). He explains that, so far as human perception is concerned, his formula for the existence of the perceivable is "conditional perception by myself or actual perception by someone else" (p. 64). But this formula, Dr. Luce has no difficulty in showing, is not ultimately self-explanatory. It pre-supposes the 'divine factor' in the human perceptual situation. "God is the home of the perceivable when it is unperceived by man". (p. 75). Now Dr. Broad takes Berkeley's teaching here to mean that God produces ideas 'telepathically' in our minds and is not so much, therefore, the 'home of the perceivable' as the permanent source of possible future perceptions. (*Proc. Brit. Ac.*, 1942, p. 122). Dr. Luce, on the other hand, fastens on

Berkeley's doctrine of God as a guarantee of the objectivity of the perceivable in a sense which makes Berkeley a realist *without jeopardy to his immaterialism*. 'Seeing all things in God' implies the existence of an order of appearances which, though subject to the New Principle (*esse est percipi*) and, therefore, wholly intra-mental, are nevertheless *always there* in the mind of God waiting to be perceived by this or that finite mind. Dr. Luce makes it plain that this is how he understands the doctrine of the divine participation. God not only 'produces' ideas, but 'offers' them to us. The world consists of "ideas perceived and ideas *waiting to be perceived*" (p. 45 ital. mine). For God's "action is action in depth, and we must not think of it as a passing, superficial impression on the mind" (p. 105). His "continuous creative action makes and sustains a vast system of significant things and *exhibits them to us*" (*Ibid.* italics mine). And, as intra-mental entities, these objects are 'sustained' by the fact that God *perceives* them. "When neither you, nor I, nor any other created spirit, perceives the object, it is still there, still luminous, still significant, still perceived, still in the mind of God" (p. 124). There is, accordingly, in Berkeley no 'intermittency' and no shutting up of percipients within their own private worlds. "You can perceive my ideas of sense, and I yours; for neither of us makes what we both see" (*ibid.*). You may see what I see and I see what you see because we are both seeing what God sees. Berkeley's *intra-mental* world is at the same time "a public world, a world in which we all may share" (p. 136).

How is Dr. Luce able to justify this paradox? By skilfully arguing as if the implication of the New Principle were that 'to be' is to be *generally* 'significant' or, in other words, that the dependence of sensible existence is upon perception *in general*. Time and again, while recognizing that mind for Berkeley means *a* mind, Dr. Luce himself uses the term in a sense which obscures the numerical distinction between the Infinite Mind and finite minds and between one finite mind and another. Thus in both my world and God's "the object is not absolute but relative *to mind*" (p. 71, italics mine) and there is "no great difference in principle between seeing all things in God and seeing all things in the mind" (p. 74). Again, it is because there is such a thing as "a spiritual Substance of the universe [that] . . . the object of sense can be handed on from man to man" (p. 124).

Now the question I should like to ask, is whether this interpretation of the New Principle is consistent with Berkeley's attack on abstract general ideas. Can Berkeley, of all people, admit the existence of 'mentality' or consciousness or perception *in general* or, therefore, equate 'being' with the fact of being perceived in general? On Dr. Luce's own showing, he is not a pantheist. God's mind, though infinite, is no less individual than men's minds. But if only individual percipients exist and all perceiving is particular, *esse est percipi* must imply that the 'being' of whatever a given percipient at any moment perceives is 'to be perceived' *in* that particular perceiving and, therefore, only *by* that particular percipient. The New Principle, in other words, is inescapably subjectivist. Does not Berkeley himself say: "the table . . . exists, *that is, I* see and feel it"? (*Princ.* III, italics mine). It follows, therefore, that since ideas exist in the mind of God only in so far as they are perceived by God, *those* ideas are internal to God's mind in a sense which must surely preclude the possibility of their constituting at the same time an order of intra-mental existence which is always there 'waiting to be perceived' by the other denizens of the world of 'mind'.

It seems to me that the only rôle God can have on Berkeley's presuppositions is that of generating *private* 'ideas' telepathically, and, therefore, necessarily *intermittently*, in the minds of finite percipients. The private worlds thus generated will be independent of our volitions and, therefore, objective in point of origin, but *not* objective in point of sensible content. All sensible content is subject to the New Principle. It can exist only in the *particular* mind which perceives it. What Berkeley's immaterialism must imply, in other words, is not that 'things' are in some sense always there, waiting to be perceived by us, but that *God* is always there waiting to 'excite' in us ideas which, before this happens are just nowhere. But is this common-sense?

J. R. JONES.

The Case for Pacifism and Conscientious Objection. By Rev. E. L. ALLEN, M.A., PhD.; F. E. POLLARD, M.A., J.P.; and G. A. SUTHERLAND, M.A. London, Central Board for Conscientious Objectors, 1946. Pp. 24, 24, 24. 3s. 6d.

THIS work is published as a reply to Prof. Field's recent booklet on *Pacifism and Conscientious Objection*, where the author had examined the arguments brought before him in the course of his experience as member of a tribunal (reviewed by me in the January number of MIND). Those who were shocked by the low standard of the objectors' arguments there will find the present statement to rise far above such a level; but in justice to Prof. Field we must remember that he was giving an account of what was actually said by the ordinary applicants, not by a select few of a considerably higher intellectual calibre such as have written this book, It may be contended that the pacifist case should be judged by its best, not by its worst, advocates; but at least, since the crude fallacies mentioned by Field have obviously exercised an influence, it is well that they should be exposed, and it seems to me that the writers of the articles now under discussion go too far in whitewashing the intelligence of conscientious objectors against such damaging imputations, though I (and, no doubt, Field) would agree with the point made in reply that a person may be a quite genuine objector even if he uses only silly arguments and that these may not represent the real basis of his conviction.

Turning to the actual statements of their case by the three writers whose articles make up the pamphlet under discussion, they certainly command respect, and in my case a considerable degree of sympathy and, I think, understanding, though it does not seem to me that they altogether face the most vital issues raised by Field or altogether escape the fallacies which he has pointed out.

In the case for the objector as stated here we can distinguish three main contentions. (1) In war all the ordinary moral principles which every decent man respects in peace are reversed in our conduct towards the enemy, thus putting war in a quite different position from any other line of action commonly sanctioned. (2) Whatever view one may hold as to the person of Christ, it is difficult to resist the conviction that the way of love and sacrifice which he expounded and exemplified is essentially a better way of dealing with wrong than that of force. (3) It is felt that, if war is ever to be abolished at all, some individuals must start by setting the example of repudiating it *in toto*, while the majority still accept it as a legitimate means of settling certain disputes.

No one who realises the gravity of the first charge will lightly reject pacifism, but in the case of the late war is it not also true that the dominance of the world by Hitler would have in practice resulted in a general repudiation and corruption of moral principles even greater, not for six years but for an indefinitely long period? This raises the question of the legitimacy of making exceptions to a good general moral principle for the sake of consequences. The first writer at least admits the possibility of exceptions, but, if exceptions are admitted at all, it is difficult to imagine a stronger case for an exception than the late war. He, however, makes a good point when he draws a distinction between a man who acts on the assumption that a given good moral principle is universal till he feels absolutely forced, much against the grain, to make an exception, and the man who always has an eye on possible exceptions and prepares systematically in advance to make them efficiently if the need arises. "I know of no moral philosopher who would argue that, because, in his opinion, it is sometimes not merely permissible but a duty to lie, therefore all men of eighteen and upwards must be trained in the art of lying so as to ensure that, should need arise, they will be able to do it efficiently and unblushingly." [1] But suppose the training of a nation for war were in the circumstances just what was needed to prevent war? Field would not himself claim that we could *prove* pacifism to be wrong, and unless we claim to know *a priori* that it is never justifiable to wage war, it must be a question of balancing the evil effects of not fighting in the particular case against the evils of war. As long as we are talking of national armies and not either of an international force representing an organisation like the United Nations or of ordinary police measures, the pacifist argument is, I think, extremely strong except in the most unusual cases, but I think that the case of 1939 was one of these unusual ones. No doubt most of what is said about the need and advantage of systematically pursuing a policy which really aims at peace and fair dealing long before war breaks out could be accepted by non-pacifists.

As regards the second point, the example of Christ, even if we grant that, if we were all like Christ, we should have found a better way of dealing with Hitler than war, it does not follow that, being what we are, in the situation of 1939, we could have stopped him without fighting or that we ought not to have fought. As regards the third point, I personally, though not a pacifist myself, feel very glad that there were some pacifists to give their uncompromising witness against war, but think it quite wrong to talk as if the example set by the total repudiation of it by some individuals could be the chief factor in stopping wars once for all. Nothing but a miracle could make any of the Great Powers pacifist as a whole, in time to end wars before they destroyed civilisation; but there is a rational hope that, without being completely pacifist, in the sense of making up their minds not to fight whatever happened, they might be sensible enough not to let their future disputes lead to war, just as most individuals avoid committing homicide, though few would be prepared to say that they would not fight another in self-defence if they were attacked and it was a question of "his life or mine." None of the articles really face Field's point that, even if it were wrong for a state ever to fight, it might still be right for individual pacifists to join in the war once it had begun, since refusal to do so would not stop the war and might prevent the better cause winning or prolong the slaughter.

[1] P. 13.

I have not left myself space to deal with the main topic discussed in the third article, *i.e.* the question of the defensibility of the position of those who sought unconditional exemption. I can only say that, while the author of the article has made such a position appear somewhat less unplausible than it does to Field, he still appears to me to have fallen into the fallacy of arguing as if the fact that a course of action was legally compulsory implied that anybody who acted in this way was so acting because it was legally compulsory (p. 9).

A. C. EWING.

Received also :—

A Pap, *The A Priori in Physical Theory*, New York, King's Crown Press ; London, G. Cumberlege, 1946, pp. 112, $2.00, 13s. 6d.

W. E. Hocking, B. Blanshard, C. W. Hendel and J. H. Randall, Jr., *Preface to Philosophy : Textbook*, New York, The Macmillan Co., 1946, pp. x + 508, $3.00.

R. E. Hoople, R. F. Piper, W. P. Tolley, *Preface to Philosophy : Book of Readings*, New York, The Macmillan Co., 1946, pp. xii + 513, $3.00.

R. Ingarden, *O Budowie Obranzu*, Cracow, Nakladem Polskiej Akademii Umiejętności, 1946, pp. 69.

R. Ingarden, *Esenijalne Zagadnienic Formy I Jej Podstawowe Pojęcia*, Cracow, Drukarnia Uniwersytetu Jagiellońskiego W Krakowie, 1946, pp. 64.

E. Frank, *Philosophical Understanding and Religious Truth*, London, Oxford University Press, 1945, pp. x + 209, 10s. 6d.

P. Vignon, *Au Souffle de l'Esprit Créateur* (Bibliothèque des Archives de Philosophie), Paris, Beauchesne et ses fils, 1946, pp. 202.

M. F. Sciacca, *Il Problema di Dio e della Religione nella Filosofia Attuale*, 2a Edizione riveduta e aumentata, Brescia, Morcelliana, 1946, pp. 379, L. 250.

W. R. Matthews, *Strangers and Pilgrims : Some Sermons Preached during the War*, London, Nisbet & Co. Ltd., 1945, pp. x + 146, 8s. 6d.

J. S. Bixler, *Conversations with an Unrepentant Liberal*, New Haven, Yale University Press ; London, G. Cumberlege, 1946, pp. x + 113, $2.00, 13s. 6d.

The Politics of Aristotle, trans., with Introduction, Notes and Appendices, by E. Barker, Oxford, Clarendon Press, 1946, pp. lxxvi + 411, 15s.

E. J. and L. Edelstein, *Asclepius : A Collection and Interpretation of the Testimonies*, 2 vols., Baltimore, Johns Hopkins Press ; London, G. Cumberlege, 1945, pp. xvii + 470 ; x + 277, $7.50, 50s.

A. Rey, *L'Apogée de la Science Technique Grecque*, Paris, A. Michel, 1946, pp. xviii + 313, 190 fr.

W. R. Inge, *Origen* (British Academy, Annual Lecture on a Master Mind, 1946), London, G. Cumberlege, 1946, pp. 23, 2s.

M. S. Fisher, *Robert Boyle, Devout Naturalist*, Philadelphia, Oshiver Studio Press, 1945, pp. 184.

M. M. Rossi, *L'Estetica dell 'Empirismo Inglese*, 2 vols., Florence, G. C. Sansoni, 1944, pp. 498, 499-969, L. 300.00.

W. C. Kvaraceus, *Juvenile Delinquency and the School*, Yonkers-on-Hudson, N.Y., World Book Company ; London, G. G. Harrap & Co. Ltd., 1945, pp. x + 337, 12s. 6d.

H. Kelsen, *Society and Nature* (International Library of Sociology and Social Reconstruction), London, Kegan Paul, 1946, pp. viii + 391, 21s.

C. Waterman, *The Three Spheres of Society*, London, Faber and Faber Ltd., 1946, pp. 294, 12s. 6d.

C. G. Weber, *Studies in the English Outlook between the World Wars*, Berne, A. Francke A.-G., 1945, pp. 189, Swiss fr. 8.

F. de Havas, *The Equilibrium Theory of the Human Consciousness*, Glasgow, M. D. Macrae, pp. 15, 2s. 8d.

J. H. Burton, *How to Study for Profit*, London, Vawser and Wiles Ltd., pp. 83, 5s.

E. B. F. Molteno, *Beyond Choice, Chance or Fate*, Cape Town, Goodwood Press, 1945, pp. 138.

E. R. Vincent, *Re-reading the Divine Comedy* (British Academy, Annual Italian Lecture, 1945), London, G. Cumberlege, 1946, pp. 15, 2s. 6d.

F. E. J. Ockenden, *Introduction to the Electron Microscope* (Monographs of the Quekett Microscopical Club), London, Williams and Norgate Ltd., 1946, pp. 24, 2s. 6d.

Giornale di Metafisica, Rivista Bimestrale di Filosifia, ed. by M. F. Sciacca, Anno I, N.1, Turin, Società Editrice Internazionale, 1946, pp. 67, Annual Subs. L. 1300, Single Numbers, L. 300.

Humanitas, A University Quarterly, Vol. I, No. 1: Summer 1946, Manchester University Union, pp. 46, Annual Subs. 10s., Single Numbers 2s. 6d.

VIII.—PHILOSOPHICAL PERIODICALS.

Revue Philosophique de Louvain. Tome 44. (3e Série, No. 2, mai 1946). **Gérard Verbeke.** *Comment Aristote conçoit-il l'immatériel.* [This question can be settled only by analysing Aristotle's account of the soul in *De Anima* and of the First Mover in the *Physics*, and relating these to other relevant passages in his works. (1) *The immaterial is indivisible, but not necessarily imperishable.* In *Physics* III, A. says that the First Mover is not material, because what is material is divisible, and therefore finite, whereas the First Mover is infinite. As an argument to prove that A. regarded all immaterial things as indivisible, this is clearly fallacious. Is there any passage where Aristotle states that what is divisible is necessarily corporeal? (2) *The immaterial may be perfectively passive, as in the reception by the intelligence of essences communicated by the senses and the imagination, but it is not destructively passive, like a material body that decomposes through collision or heat.* (*De Anima*, I and II.) (3) *The immaterial is self-conscious.* In finite intelligences, self-consciousness is evoked by contemplation of the outside world, but in the First Mover it is spontaneous, and not accompanied by any other activity. (*De Anima*, III. *Metaphysics*, XII.) This seems to be another paralogism. Despite these flaws, this article is a very valuable contribution to the interpretation of Aristotle.] **Robert Feys:** *La technique de la logique combinatoire* (suite et fin) [Feys now passes on to consider products of the simple combinators I, W, C, and B, and lays down rules for their arrangement. The distinctive feature of the system is that these combinators operate upon their successors according to fixed rules. whether those successors be variables or other combinators. All but I begin operation upon the second term to the right, except when conjugated with B, B^1, B^2 . . . , when they skip 2, 3, 4, or more places. Thus BC is the operation upon $\phi\alpha\beta\gamma\delta$ which results in $\phi\alpha\beta\delta\gamma$ ($\lambda\phi\alpha\beta\gamma\delta . \phi\alpha\beta\delta\gamma$), because B skips C and links $\phi\alpha$, then C skips B and commutes $\gamma\delta$. By an ingenious trick, combinators that operate on the first term of the initial sequence are also defined. Thus $\lambda\phi . \phi\phi$ is defined by WI and $\lambda\phi\alpha . \alpha\phi$ by CI. This technique makes it possible to define all the logical operations of *Principia Mathematica* with the help of a few logical constants, like negation, conjunction, alternation and implication. Going back to pure combinations, Feys defines an operator S (= ΦBI, where $\Phi = (B^2WxBB)$.) which transforms any given number into its successor, and enables him to define recursive functions without using mathematical induction. After this there follow rules for translating any λ-function by variables and combinators, then translations of general propositions about classes and relations, and then translations of the fundamental logical constants Negation, Conjunction, Alternation and Equivalence. Finally, Feys introduces a fifth operator K, which he defines as $\lambda\phi\alpha . \phi$ and calls an Eliminator. The chief importance of K seems to lie in the fact that it enables him to define O. It would be difficult to imagine a more perfect introduction to this branch of logic, and the *Revue* is to be congratulated on the courageous act of publishing it.] **Werner Peeters.** *Hillel ben Samuel, philosophe du XIIIe siècle.* [An

attempt to estimate the influence of Christian philosophy on Jewish philosophy by studying a representative Jewish philosopher who drew freely from Latin sources. Hillel ben Samuel (*c* 1230–*c* 1300) was an Italian Jew, who studied at Barcelona and Montpellier, moved about Italy considerably, and wrote a treatise on the soul entitled *Tagmule Hannefes*. Peeters agrees with Neumark that the pre-occupation of Jewish philosophy after this time with psychological problems was due to Hillel. Despite his own orthodoxy, H. played a part in persuading the Jews not to treat Maimonides (1135-1204) as an heretical writer, thus helping to save subsequent Jewish thinkers from complete absorption in the Cabala.] ÉTUDES CRITIQUES. [Includes a rather tendencious review of J.-P. Sartre's *L'existentialisme est un humanisme* by A. de Waelhens.]. CHRONIQUES.

IX.—NOTES.

PEIRCE SOCIETY.

THE next meeting of the Charles S. Peirce Society will be held during the Christmas recess in New Haven, Connecticut, in connection with the annual meeting of the American Philosophical Association. Papers will be read by Prof. Thomas Goudge, of the University of Toronto, and by Prof. Philip Wiener, of City College, New York. Other papers are solicited, and should be submitted to Dr. Paul Weiss, 31 Everit Street, New Haven, Connecticut, before November 1st.

FREDERIC H. YOUNG,
Secretary of Peirce Society.

MIND ASSOCIATION: REPORT OF ANNUAL MEETING.

The Forty-sixth Annual General Meeting of the Mind Association was held on 7th July in Hulme Hall, Manchester, the President, Mr. R. B. Braithwaite in the chair. The Treasurer's Report was adopted. The President announced that the next meeting would probably be in Cambridge. Professor C. D. Broad was elected President for the ensuing year. The Executive Committee reported that Mr. H. L. A. Hart had been elected Secretary.

The Secretary reported appeals from war-damaged libraries in Europe and it was agreed to supply these with back numbers of MIND so far as existing stocks permit.

There were present at the meeting Professors Baier, Berger and Leroy from France, and Professor De Vaux from Belgium, who were welcomed by the President.

MIND ASSOCIATION.

Those who wish to join the Association should communicate with the Hon. Secretary, Mr. H. L. A. HART, New College, Oxford; or with the Hon. Treasurer, Mr. H. STUBT, 55 Park Town, Oxford, to whom the yearly subscription of sixteen shillings should be paid. Cheques should be made payable to the Mind Association, Westminster Bank, Oxford. Members may pay a Life Composition of £16 instead of the annual subscription.

In return for their subscriptions members receive MIND gratis and post free, and (if of 3 years' standing) are entitled to buy back numbers of both the Old and the New Series at half-price.

Members resident in U.S.A. may pay the subscription ($4) to the Hon. Assistant-Treasurer, Prof. B. Blanshard, Dept. of Phil., Yale University, New Haven, Conn.